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PHILOSOPHICAL TRANSACTIONS,

OF THE

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OF

LONDON.

VOL. LXXV. For the Year 1785.

PART I.

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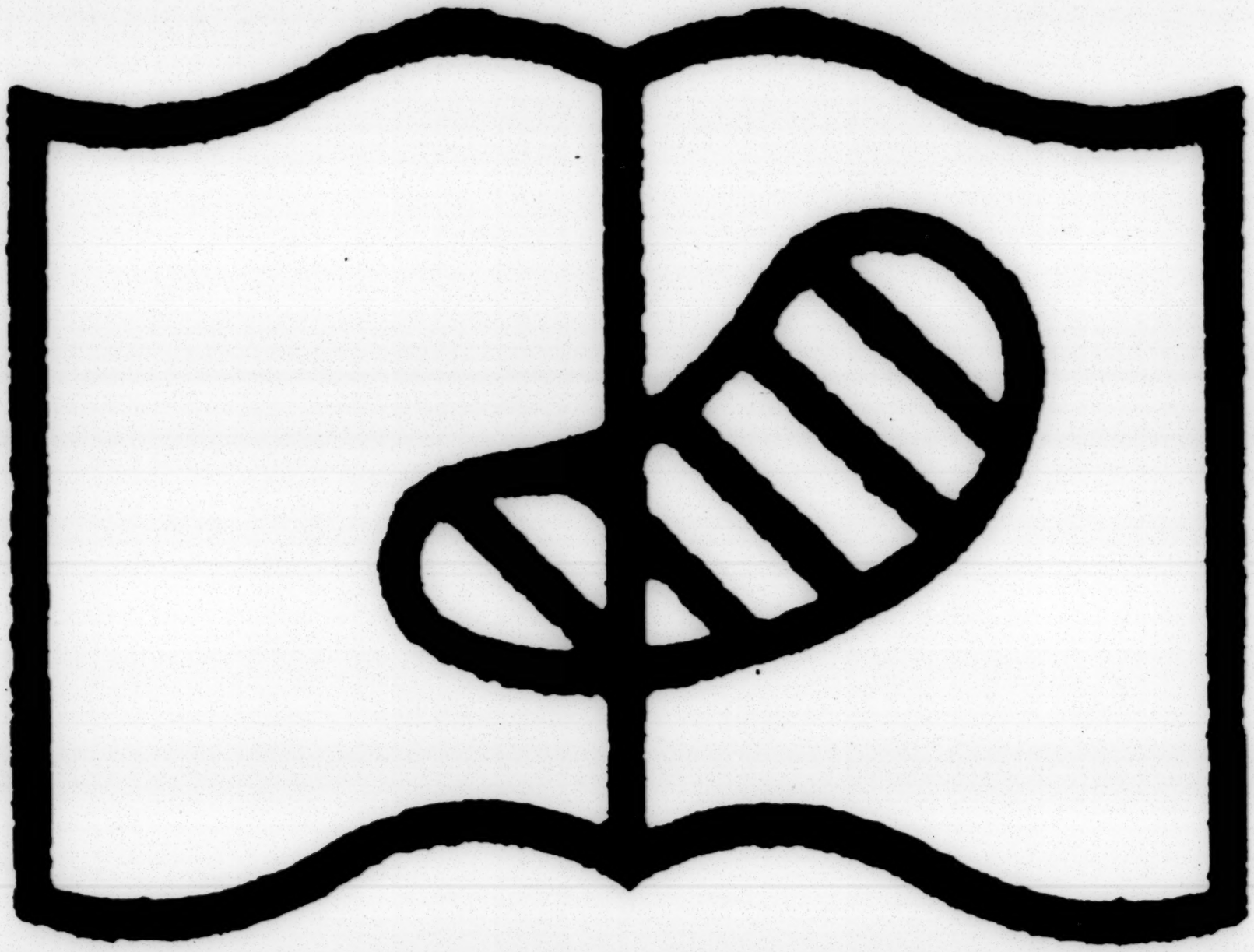

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THE Committee appointed by the *Royal Society* to direct the publication of the *Philoſophical Tranſactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in ſeveral former *Tranſactions*, that the printing of them was always, from time to time, the ſingle act of the reſpective Secretaries, till the Forty-ſeventh Volume: the Society, as a Body, never intereſting themſelves any further in their publication, than by occaſionally recommending the revival of them to ſome of their Secretaries, when, from the particular circumſtances of their affairs, the *Tranſactions* had happened for any length of time to be intermitted. And this ſeems principally to have been done with a view to ſatisfy the Public, that their uſual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their firſt inſtitution by the Royal Charters, and which they have ever ſince ſteadily purſued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviſable, that a Committee of their members ſhould be appointed to reconſider the papers read before them, and ſelect out of them ſuch, as they ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

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THE Preſident and Council of the Royal Society adjudged, for the Year 1784, the Medal on Sir GODFREY COPLEY's Donation, to EDWARD WARING, M. D. Lucaſian Profeſſor of the Mathematics at Cambridge, for his Mathematical Communications to the Society.

PHILOSOPHICAL TRANSACTIONS.

I. *An Account of an artificial Spring of Water.* *By* Erasmus Darwin, *M. D. F. R. S.*

Read November 4, 1784.

To the President and Fellows of the Royal Society.

GENTLEMEN, Derby, July 16, 1784.

CONFIDENT that every atom which may contribute to increase the treasury of useful knowledge, which you are so successfully endeavouring to accumulate, will be agreeable and interesting to the Society, I send you an account of an artificial spring of water, which I produced last summer near the side of the river Darwent in Derby.

Near my houſe was an old well, about one hundred yards from the river, and about four yards deep, which had been many years diſuſed on account of the badneſs of the water, which I found to contain much vitriolic acid, with, at the ſame time, a ſlight ſulphureous ſmell and taſte; but did not carefully analyſe it. The mouth of this well was about four feet above the ſurface of the river; and the ground, through which it was ſunk, conſiſted of a black, looſe, moiſt earth, which appeared to have been very lately a moraſs, and is now covered with houſes built upon piles. At the bottom was found a bed of red marl, and the ſpring, which was ſo ſtrong as to give up many hogſheads in a day, oozed from between the moraſs and the marl: it lay about eight feet beneath the ſurface of the river, and the water roſe within two feet of the top of the well.

Having obſerved that a very copious ſpring, called Saint Alkmund's well, roſe out of the ground about half a mile higher on the ſame ſide of the Darwent, the level of which I knew by the height of the intervening wier to be about four or five feet above the ground about my well; and having obſerved, that the higher lands, at the diſtance of a mile or two behind theſe wells, conſiſted of red marl like that in the well; I concluded, that, if I ſhould bore through this ſtratum of marl, I might probably gain a water ſimilar to that of St. Alkmund's well, and hoped that at the ſame time it might riſe above the ſurface of my old well to the level of St. Alkmund's.

With this intent a pump was firſt put down for the purpoſe of more eaſily keeping dry the bottom of the old well, and a hole about two and an half inches diameter was then bored about thirteen yards below the bottom of the well, till ſome ſand was brought by the auger. A wooden pipe,

which was previouſly cut in a conical form at one end, and armed with an iron ring at the other, was driven into the top of this hole, and ſtood up about two yards from the bottom of the well, and being ſurrounded with well-rammed clay, the new water aſcended in a ſmall ſtream through the wooden pipe.

Our next operation was to build a wall of clay againſt the moraſſy ſides of the well, with a wall of well-bricks internally, up to the top of it. This completely ſtopped out every drop of the old water; and, on taking out the plug which had been put in the wooden pipe, the new water in two or three days roſe up to the top, and flowed over the edges of the well.

Afterwards, to gratify my curioſity in ſeeing how high the new ſpring would riſe, and for the agreeable purpoſe of procuring the water at all times quite cold and freſh, I directed a pipe of lead, about eight yards long, and three-quarters of an inch diameter, to be introduced through the wooden pipe deſcribed above, into the ſtratum of marl at the bottom of the well, ſo as to ſtand about three feet above the ſurface of the ground. Near the bottom of this leaden pipe was ſewed, between two leaden rings or flanches, an inverted cone of ſtiff leather, into which ſome wool was ſtuffed to ſtretch it out, ſo that, after having paſſed through the wooden pipe, it might completely fill up the perforation of the clay. Another leaden ring or flanch was ſoldered round the leaden pipe, about two yards below the ſurface of the ground, which, with ſome doubles of flannel placed under it, was nailed on the top of the wooden pipe, by which means the water was perfectly precluded from riſing between the wooden and the leaden pipes.

This being accompliſhed, the bottom of the well remained quite dry, and the new water quickly roſe about a foot above the top of the well in the leaden pipe; and, on bending the mouth of this pipe to the level of the ſurface of the ground, about two hogſheads of water flowed from it in twenty-four hours, which had ſimilar properties with the water of St. Alkmund's well, as on compariſon both theſe waters curdled a ſolution of ſoap in ſpirit of wine, and abounded with calcareous earth, which was copiouſly precipitated by a ſolution of fixed alkali; but the new water was found to poſſeſs a greater abundance of it, together with numerous ſmall bubbles of aërial acid or calcareous gas.

The new water has now flowed about twelve months, and, as far as I can judge, is already increaſed to almoſt double the quantity in a given time; and from the rude experiments I made, I think it is now leſs replete with calcareous earth, approaching gradually to an exact correſpondence with St. Alkmund's well, as it probably has its origin between the ſame ſtrata of earth.

As many mountains bear inconteſtible marks of their having been forcibly raiſed up by ſome power beneath them; and other mountains, and even iſlands, have been lifted up by ſubterraneous fires in our own times, we may ſafely reaſon on the ſame ſuppoſition in reſpect to all other great elevations of ground. Proofs of theſe circumſtances are to be ſeen on both ſides of this part of the country; whoever will inſpect, with the eye of a philoſopher, the lime-mountain at Breedon, on the edge of Leiceſterſhire, will not heſitate a moment in pronouncing, that it has been forcibly elevated by ſome power beneath it; for it is of a conical form, with the apex cut off,

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and the ſtrata, which compoſe the central parts of it, and which are found nearly horizontal in the plain, are raiſed almoſt perpendicularly, and placed upon their edges, while thoſe on each ſide decline like the ſurface of the hill; ſo that this mountain may well be repreſented by a bur made by forcing a bodkin through ſeveral parallel ſheets of paper. At Router, or Eagle-ſtone, in the Peak, ſeveral large maſſes of grit-ſtone are ſeen on the ſides and bottom of the mountain, which by their form evince from what parts of the ſummit they were broken off at the time it was elevated; and the numerous looſe ſtones ſcattered about the plains in its vicinity, and half buried in the earth, muſt have been thrown out by exploſions, and prove the volcanic origin of the mountain. Add to this the vaſt beds of toad-ſtone or lava in many parts of this county, ſo accurately deſcribed, and ſo well explained, by Mr. WHITEHURST, in his Theory of the Formation of the Earth.

Now as all great elevations of ground have been thus raiſed by ſubterraneous fires, and in a long courſe of time their ſummits have been worn away, it happens, that ſome of the more interior ſtrata of the earth are expoſed naked on the tops of mountains; and that, in general, thoſe ſtrata, which lie uppermoſt, or neareſt to the ſummit of the mountain, are the loweſt in the contiguous plains. This will be readily conceived if the bur, made by thruſting a bodkin through ſeveral parallel ſheets of paper, had a part of its apex cut off by a penknife, and is ſo well explained by Mr. MICHELL, in an ingenious paper on the Phænomena of Earthquakes, publiſhed a few years ago in the Philoſophical Tranſactions.

And as the more elevated parts of a country are ſo much colder than the vallies, owing, perhaps, to a concurrence of

two or three cauſes, but particularly to the leſs condenſed ſtate of the air upon hills, which thence becomes a better conductor of heat, as well as of electricity, and permits it to eſcape the faſter; it is from the water condenſed on theſe cold ſurfaces of mountains, that our common cold ſprings have their origin; and which, ſliding between two of the ſtrata above deſcribed, deſcend till they find or make themſelves an outlet, and will in conſequence riſe to a level with the part of the mountain where they originated. And hence, if by piercing the earth you gain a ſpring between the ſecond and third, or third and fourth ſtratum, it muſt generally happen, that the water from the loweſt ſtratum will riſe the higheſt, if confined in pipes, becauſe it comes originally from a higher part of the country in its vicinity.

The increaſing quantity of this new ſpring, and its increaſing purity, I ſuppoſe to be owing to its continually diſſolving a part of the earth it paſſes through, and hence making itſelf a wider channel, and that through materials of leſs ſolubility. Hence it is probable, that the older and ſtronger ſprings are generally the purer; and that all ſprings were originally loaded with the ſoluble impurities of the ſtrata, through which they tranſuded.

Since the above-related experiment was made, I have read with pleaſure the ingenious account of the King's wells at Sheerneſs, in the laſt volume of the Tranſactions, by Sir Thomas Hyde Page, in which the water roſe three hundred feet above its ſource in the well; and have alſo been informed, that in the town of Richmond, in Surrey, and at Inſhip near Preſton in Lancaſhire, it is uſual to bore for water through a lower ſtratum of earth to a certain depth; and that when it

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is found at both thoſe places, it riſes ſo high as to overflow the ſurface of the well: all theſe facts contribute to eſtabliſh the theory above-mentioned. And there is reaſon to conclude, that if ſimilar experiments were made, artificial ſprings, riſing above ground, might in many places be thus produced at ſmall expence, both for the common purpoſes of life, and for the great improvement of lands by occaſionally watering them.

II. *An Account of an* Engliſh *Bird of the Genus* Motacilla, *ſuppoſed to be hitherto unnoticed by* Britiſh *Ornithologiſts; obſerved by the Rev.* John Lightfoot, *M. A. F. R. S. In a Letter to Sir* Joſeph Banks, *Bart. P. R. S.*

Read November 18, 1784.

SIR, Uxbridge, Nov. 20, 1783.

AS every diſcovery in natural hiſtory is eſteemed worthy the notice of that Society which was inſtituted on purpoſe to improve natural knowledge, I have taken the liberty to ſend you a deſcription and drawing of a bird which haunts the reeds of the river Coln, in the neighbourhood of Uxbridge, and which ſeems to have hitherto eſcaped the notice of writers on Britiſh Ornithology; and therefore ſome account and deſcription of it will not, I truſt, be unacceptable to the Society over which you ſo laudably preſide.

The neſt and eggs of the bird I am about to deſcribe firſt attracted my attention, and led to the diſcovery of the bird itſelf. They were repeatedly brought by a fiſherman on the Uxbridge river, in the pariſh of Denham, to her grace the Ducheſs Dowager of Portland, who firſt communicated them to me. They were ſuppoſed by the fiſherman to belong to the *Sedge-bird* of PENNANT, or *Motacilla Salicaria* of LINNÆUS; but being well acquainted with the neſt and eggs of this, I was very ſure he was miſtaken, though he actually produced this bird as the true proprietor of the ſubjects in queſtion. The ſtructure and poſition of the neſt having a ſingular appearance, and both

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that and the eggs belonging to a bird unknown to me, I became deſirous of finding out the ſecret architect, and to that end made uſe of ſuch means as I thought moſt likely to promote the diſcovery.

In a ſhort time my expectations were gratified; for on the 26th day of July, 1783, intelligence was brought me, that ſuch a neſt as I wanted was found. I had given previous direction, that it ſhould not be diſturbed before I had ſeen it. Upon examination, I inſtantly perceived it to be of the ſame kind and ſtructure with that under enquiry, containing two eggs, and two young ones juſt excluded from the ſhell. One of the old birds was ſitting at this time upon the neſt, which a perſon in company attempting to ſeize, it flew at him with ſo much reſentment and acrimony, as to draw blood from the hand that dared to moleſt its inſtinctive operations. Both the parent birds continued hovering about their neſt with much watchful care and anxiety, while I made ſeveral attempts to take them alive; but, finding all endeavours in vain, leſt I ſhould loſe the opportunity of examining them with accuracy, I at length, with reluctance, cauſed them to be ſhot. From theſe ſpecimens the following deſcriptions were made, which, with an accurate drawing of one of them, together with its neſt and egg, are humbly ſubmitted to your notice.

From the generic characters delivered by LINNÆUS, our bird muſt evidently be reduced to the family of his *Motacilla*, for it has a weak, ſlender, ſubulate bill, almoſt ſtraight; the mandibles nearly equal; the noſtrils oval and naked, or not covered with briſtles; the tongue lacerated at the extremity; the legs ſlender; the toes divided to the origin, except that the exterior one is joined, at the under part of the laſt joint, to the middle toe; the claws of nearly equal length.

The male and female have the ſame coloured plumage, ſo that one deſcription will ſerve for both. They differ a little in ſize, but their external appearance is the ſame. They are both larger than the *Pettychaps* deſcribed by WILLOUGHBY; ſmaller than the *White-throat*, and nearly of the ſame ſize with the *Willow-wren*; but to be more particular.

The cock-bird weighed, when juſt killed, exactly ſeven pennyweights and nine grains; the hen ſix pennyweights and nine grains, or one pennyweight leſs.

The males meaſured, from tip to tip of the extended wings, ſeven inches and a half; the female ſix and three-quarters.

From the end of the bill to the extremity of the tail, the cock meaſured five inches and a half; the hen only five inches.

The bill in both meaſured half an inch, which is longer in proportion than in moſt of this genus. The *upper* mandible is of a dark horn colour, ſlightly incurved near the extremity, with a minute indenture on either ſide near the point; the *lower* is pale red or fleſh-coloured, with a ſhade of yellow; the inſide of the mouth deep orange-coloured; the tip of the tongue cloven and ciliated; the noſtrils oval, and deſtitute of a briſtly covering; but at the baſe of the upper mandible, on either ſide, near the angle of the mouth, ariſe three ſhort *vibriſſæ* pointing downwards, black at their ſummits, white at their baſes; a circumſtance common to many others of this genus. The *iris* of the eye is olive-brown; the pupil black. The ſhort feathers of the orbits or eye-laſhes are of a dirty white colour. From the corner of each eye to the noſtril is a broad ſtroke or band of tawny-white feathers, lying over each other, and running narroweſt towards the bill; this affords an excellent mark to diſtinguiſh the ſpecies.

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The feathers of the head, neck, back, coverts of the wings and rump, are of an olive-brown, with a ſlight tinge of green. The quill and tail feathers are all of a darker hue, or ſimply brown; their outward edges of a paler ſhade. The tail is two inches long, ſlightly cuneated, the middle feathers being a little longer than the reſt, the others gradually ſhorter; all of one uniform dun-brown colour edged with paler brown, and a little wedge-ſhaped at their ends.

The chin is white; the throat, breaſt, belly, and parts about the vent, are white with a ſlight ſhade of buff or tawny; but all theſe feathers (as in ſeveral others of this genus) when blown aſunder, or cloſely examined, are found to have their baſe or lower half black, except the ſhafts, which are white throughout.

The ridge and under coverts of the exterior angle of the wing are of a yellowiſh-tawny colour, as are alſo the feathers of the thighs; but thoſe of the knees are a ſhade darker, or a pale yellowiſh brown.

The legs are a light olive; the ſoles of the feet bright yellow, with a tinge of green, which ſoon fades after the bird is dead. The inſtep is covered with ſeven large imbricated ſcales, and five ſmaller on the toes, as in others of the genus. The toes ſtand three before, and one behind: the claws are nearly of equal length and curvature; but the hindmoſt is thickeſt and ſtrongeſt.

From the foregoing remarks it is evident, that the bird mentioned is a ſpecies of *Motacilla*, which, as I can find no ſuch deſcribed by any ſyſtematic writer, I ſhall venture to name, after the LINNÆAN manner,

Motacilla (*arundinacea*) ſupra olivaceo-fuſca, ſubtus albida, loris et orbitis fuſco-albeſcentibus, angulo carpi ſubtus

luteo-fulvo, cauda ſubcuneata fuſca, plantis luteo-vireſcentibus.

In regard to ſynonyms, the only author I can find who can be ſuſpected of having noticed this bird is SEPP, who, in a late ſplendid work, in the Dutch language, intituled, Nederlandſche Vogelen *(fol. chart. max.)* p. 101. has deſcribed and figured a bird, under the name of *Turdus arundinaceus minimus*, called in Holland *Karrakietje*, which in many reſpects agrees with our bird; but as the colour of the wings in that figure is made a reddiſh brown, inſtead of an olive-brown, and the tawny-white *Lora* (a moſt eſſential character to diſtinguiſh the ſpecies) are not at all expreſſed; and the eggs are made to be of a pale-bluſh colour with dark ſpots, inſtead of a dirty-white with olive ſpots; I cannot pronounce for certain, that the bird there intended by that writer is the ſame which we have now deſcribed; though, if ſome allowance be made for ill-colouring and other omiſſions, it may poſſibly have been deſigned for the ſame ſpecies.

As we have already a bird, called in Engliſh the *Willow-wren*; ours, being nearly of the ſame ſize and ſhape, as well as the ſame genus, may, from its haunts, not improperly be denominated the *Reed-wren*.

It frequents the banks of the river Coln near Uxbridge, as far as from Harefield-Moor down to Iver, about the ſpace of five miles, and very probably moſt other parts of the ſame river, though not as yet obſerved.

It is alſo certainly found in the neighbourhood of Dartford in Kent, from whence a neſt and eggs were communicated by the ingenious Mr. LATHAM of that place, but without knowledge of the bird to which they belonged; ſo that there is little doubt but that it may be found in many parts of the kingdom.

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Its food is inſects, at leaſt in part, for I obſerved it catching flies. It hops continually from ſpray to ſpray, or from one reed to another, putting itſelf into a ſtooping poſture before it moves. I heard it make no other than a ſingle note, not unlike the ſound of the word *peep*, uttered in a low plaintive tone; but this might probably be only a note of diſtreſs, and it may have, perhaps, more pleaſing and melodious ones at other times, with which I am unacquainted.

The neſt of this bird is a moſt curious ſtructure, unlike that of any other I am acquainted with, enough to point out the difference of the ſpecies, if every other character was wanting.

It may not be amiſs here to obſerve, that there is ſuch a manifeſt diverſity in the materials, locality, and formation of neſts, and ſuch variety of colours in the eggs of many birds (in other reſpects hard to be diſtinguiſhed), that it is pity this part of Ornithology has not been more attended to. I am well convinced, that as many ſpecies of *inſects*, nearly allied to each other in colours and ſhape, and reputed to be only *varieties*, are frequently, from a due attention to their *larvæ* (which are often extremely different), diſcovered to be ſpecies *totally diſtinct*; ſo, amongſt *birds* of ſimilar genus and feather, their true differences may be often found by carefully obſerving their neſts and eggs, when other characters are ſo minute, in the birds themſelves, as to be diſtinguiſhed with difficulty. By experience I have found this to be remarkably verified in ſome of the Lark kind.

But to return to the neſt I was going to deſcribe. It is compoſed externally of dry ſtalks of graſs, lined, for the moſt part, with the flowery tufts of the common reed, or *Arundo vallatoria*, but ſometimes with ſmall dead graſſes, and a few black horſe-hairs to cover them. This neſt is uſually found

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ſuſpended or faſtened on, like a hammock, between three or four ſtalks of reeds, below the panicles of flowers, in ſuch a manner that the ſtalks run through the ſides of the neſts at nearly equal diſtances; or, to ſpeak more properly, the neſt is tied on to the reeds with *dead graſs*, and ſometimes (as being more eligible when it can be had) even with *thread* and *pack-thread*, emulating the work of a ſempſtreſs, as was the caſe of the neſt exhibited in the drawing. The bird, however, though generally, does not always confine her building to the ſupport of reeds; ſometimes ſhe fixes it on to the branches of the *Water-dock*; and, in one inſtance only (that here delineated), it was found faſtened to the trifurcated branch of a *Syringa* buſh, or *Philadelphus*, growing in a garden hedge by the river ſide.

She lays commonly four eggs; the ground colour a dirty white, ſtained all over with dull olive-coloured ſpots, but chiefly at the greater end, where are generally ſeen two or three ſmall irregular black ſcratches; but theſe are ſometimes ſcarcely viſible.

I muſt not omit, that both the neſt and eggs which I have now deſcribed, whether deſigned for the ſame or not, are well expreſſed by SEPP, in the work above cited, under the article *Turdus Calamoxenus*, or *Rietvinck*, p. 97.; but as the bird there repreſented is evidently the *Motacilla Sylvia*, LIN. or common *White-throat* (which is known to make a very different neſt), I am inclined to believe, that the author, by miſtake, placed a bird and neſt in the ſame plate which do not belong to each other.

I have reaſon to think, that the bird I have been characterizing is a bird of migration; for the inhabitants on the ſides

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of the Coln do not recollect ever to have ſeen it in the winter months; and its food being inſects, it is probable, it muſt be obliged to ſhift its quarters for a warmer climate at the approach of a ſevere ſeaſon; but this at preſent is only matter of conjecture, and not certainty.

I am, &c.

JOHN LIGHTFOOT.

III. *An Account of* Morne Garou, *a Mountain in the Iſland of* St. Vincent, *with a Deſcription of the Volcano on its Summit. In a Letter from Mr.* James Anderſon, *Surgeon, to Mr.* Forſyth, *His Majeſty's Gardener at* Kenſington; *communicated by the Right Honourable Sir* George Yonge, *Bart. F. R. S.*

Read November 18, 1784.

THE many ridges of mountains which interſect this iſland in all directions, and riſe in gradations, one above the other, to a very great height, with the rivers tumbling from their ſides over very high precipices, render it exceeding difficult to explore its interior parts.

The moſt remarkable of theſe mountains is one that terminates the N.W. end of the iſland, and the higheſt in it, and has always been mentioned to have had volcanic eruptions from it. The traditions of the oldeſt inhabitants in the iſland, and the ravins at its bottom, ſeem to me to vindicate the aſſertion

As I was determined, during my ſtay in the iſland, to ſee as much of it as I could; and as I knew, from the altitude of this mountain, there was a probability of meeting with plants on it I could find in no other part of the iſland; I ſhould have attempted going up if I had heard nothing of a volcano being on it. But viewing the mountain at a diſtance, the ſtructure of it was different from any in the iſland, or any I had ſeen in the Weſt Indies. I could perceive it divided into many

different ridges, ſeparated by very deep chaſms, and its ſummit appeared quite deſtitute of any vegetable production. On examining ſeveral ravins, that run from the bottom a great way up the mountain, I perceived they were quite deſtitute of water, and found pieces of pumice-ſtone, charcoal, ſeveral earths and minerals, that plainly indicated there muſt be ſome very ſingular place or other on ſome part of the mountain. I alſo recollected a ſtory told by ſome very old men in the iſland, that they had heard the captain of a ſhip ſay, that between this iſland and St. Lucia he ſaw, towards night, flames and ſmoke iſſuing from the top of this mountain, and next morning his decks were covered with aſhes and ſmall ſtones. This, you may readily imagine, was excitement enough to examine it, if I poſſibly could; but I was much diſcouraged upon being told, it was impoſſible to gain the ſummit of it; nor could I get either white men, Carribbee, or Negro, that would undertake to conduct me up for any reward I could offer; nor could I get any information relative to it. But as difficulty to attain inhances the value of the object, ſo the more I was told of the impoſſibility of going up, the more was I determined to attempt it.

After I had examined the baſis of it, as far as I could for the ſea and other mountains, to find the moſt probable place to commence my journey, I obſerved an opening of ſeveral large and dry ravins, that ſeemingly ran a great way up; but I was not ſure if they were not interſected by ſome rocks or precipices I could not get over. I came to Mr. MALOUNE's, about a mile diſtant from the mountain, but the nigheſt houſe to it I could ſtay at all night. Here I met with a friendly reception and great hoſpitality. After communicating my intentions to him, he told me, he would give me every aſſiſtance

he could, by ſending ſome truſty negroes with me, and wiſhed he was able to go with me himſelf. This was a kind offer to me, in my then ſituation, as negroes were what I only wanted, having only one boy belonging to Dr. YOUNG with me. I knew, if I had great difficulties in the woods, he and I both ſhould be inadequate to the taſk, as in a ſhort time we ſhould be ſo wearied as to be unable to proceed: from what I had ſeen of the mountain, I knew I muſt be under the neceſſity of carrying water with me; and from the great diſtance to the top, and obſtructions we might naturally expect, I ſhould at leaſt require two days to accompliſh it.

By examining the ſide of the mountain towards me with a good glaſs, I imagined I ſaw two ridges I might get up. I perceived they were covered great part of the way with thick wood; yet I hoped, with a little cutting, I ſhould be able to ſcramble through them. I appointed next morning to begin my route by one of theſe ridges.

February 26, 1784, I left Mr. MALOUNE's about ſun-riſe, with two ſtout negroes and Dr. YOUNG's boy; each of us having a good cutlaſs, as well to clear our way through the woods, as to defend us in caſe we ſhould be attacked by Caribbees or run-away negroes. We arrived at the bottom of the mountain a little before ſeven in the morning. To get to either of the ridges, we found we had a rock to climb above forty feet high: it was with great difficulty we ſcrambled up, aſſiſting one another in the beſt manner we could; here we found it neceſſary to contract our baggage. After getting up this rock, I found myſelf in the bottom of a narrow and deep ravin. Having aſcended this ravin a little way, I ſaw ſome cleared ground on its ſides, with tobacco growing. This I conjectured was the habitation of ſome Caribbees; but I was much

ſurpriſed

furpriſed when one of the negroes I had with me told me, it was the habitation of a Mr. GASCO, a Frenchman. What could induce a ſtout healthy man in the prime of life, and a good mechanick, with ſeveral negroes, to take up his reſidence among rocks and precipices, excluded from the whole world, is a myſtery to me. Beſides, by every torrent of rain that happens, he may expect himſelf and all his habitation to be waſhed over the rocks into the ocean. Notwithſtanding his ſingular ſituation, I found him an intelligent man, and I experienced every hoſpitality his poor cottage could afford.

The difficulty of going through woods in the Weſt Indies, where there are no roads or paths, is far beyond any thing an European can conceive. Beſides tall trees and thick underwood, there are hundreds of different climbing plants twiſted together like ropes, and running in all directions to a great extent, and even to the tops of the higheſt trees; by puſhing on they cannot be broke, and many of them with difficulty cut; beſides a ſpecies of graſs, the *Schoenus Lithoſpermos*, with ſerrated leaves, that cuts and tears the hands and face terribly. With ſuch obſtructions as theſe it was above two hours before we got on the ridge, where I was in hopes our paſſage would have been eaſier; but I ſoon found my miſtake, for I was ſurrounded with a thick foreſt, much more difficult to get through than before, on account of the large piles of trees broken down by the hurricanes, to paſs which in many parts we were obliged to creep on our hands and feet to get below them, and in other places to climb a great height above the ſurface of the ground, to get over large trunks lying on one another, and theſe being frequently rotten, occaſioned us to tumble headlong down to a great depth, among rotten wood and graſs, ſo that it was with great difficulty I and the negroes could extricate ourſelves. By

conſtantly cutting to clear our way, I, as well as my companions, grew much fatigued, and they wiſhed much to return back. About four in the afternoon I could not prevail upon them to proceed farther; if they did, they could not return before dark, and they would not ſleep all night in the woods; but ſaid, if I ſtayed they would return to me next morning. I ſaw it was impoſſible to gain the ſummit of the mountain with the boy only by that route: I likewiſe ſaw the woods growing more difficult, my water alſo totally expended: from theſe conſiderations I intended to go down to the Frenchman's, and remain there all night, and try another route with my boy next morning, hoping I might be fortunate enough to find an eaſier paſſage. I arrived at Mr. GASCO's a little after ſun-ſet, being much fatigued and thirſty, and never experienced more hoſpitality and kindneſs than from this man in his miſerable cot; for we ought not to judge of the value of the things received, but of the diſpoſition of the heart with which they are given. He parted with his hammock to me, and ſlept on a board himſelf. This I at firſt refuſed; but he inſiſted on it, telling me, from my hardſhips of the day I was much more tired than he. I took the hammock, but I found it was impoſſible to cloſe my eyes during the night with cold. His hut was built of *roſeaux* or large reeds, between each of which a dog might creep through, and the top was covered with dry graſs. It is ſituated in the bottom of a deep gully, where the ſun does not ſhine till nine in the morning, nor after four in the afternoon. It is ſurrounded by thick wood, and during the night the whole of the mountain is covered with thick clouds, from which it frequently rains; this makes the night air exceedingly cold. I got ready to renew my journey next morning, having only Dr. YOUNG's boy with me, who continued

very faithful to me during this excurſion, being very active and hardy: I do not know if I could have gone through this fatigue had it not been for his aſſiſtance. I now determined to commence this day's route up the ravin, as it ſeemed to widen and apparently run a conſiderable way up in the direction I wiſhed for; and if I could get out of it upon the other ridge, it would at leaſt be two miles nearer than the way I had attempted yeſterday, and probably, after getting out of it, I might find wood eaſier of acceſs. In this ravin I got up about a mile and a half, without meeting with any conſiderable obſtruction. Encouraged by getting ſo far, although the ravin was narrowing faſt, with numbers of rocks and precipices to climb over, with vines and buſhes difficult to get through, I was reſolved to perſiſt in this route, and determined by every poſſible means to get to the object of my wiſhes, well knowing if I could not perform it this way, I might abandon it entirely. After climbing over a number of difficult paſſes, the ravin terminated at the bottom of a very high precipice; how far it was to the ſummit I did not know, being covered toward the top with thick wood; but from the bottom upwards it was looſe ſand as far as I could ſee, with ferns and tufts of graſs, which, as ſoon as I took hold of them, came out at the roots. The precipice being ſo very ſteep, with no trees or buſhes on it to aſſiſt me in getting up, I plainly ſaw the attempting to climb it was at the riſk of my life: however, I was reſolved to try it, and telling the boy to keep ſome diſtance behind me, in caſe I ſhould tumble and drive him down along with me, I began to aſcend, holding the tufts of graſs as lightly as poſſible, and digging holes with my cutlaſs to put my feet in; but I often loſt my hold, and frequently ſlipped down a conſiderable diſtance: however, as it was nothing but

looſe

looſe ſand, I could eaſily puſh my cutlaſs into it to the handle, and by graſping it could recover myſelf again. Had I not taken the reſolution before I began to aſcend to diveſt myſelf of fear, I could not poſſibly have gone, for the terror of falling would have been the means of it every inſtant. I got up to ſome wild plantains, which I ſaw continued all the way to the place where the buſhes and trees began to grow. I here reſted myſelf, and waited for the boy's getting to me, which he did much eaſier than I, although he had the proviſions and water, owing to the track I had made, and becauſe, being much lighter, he could better truſt himſelf to the graſs and ferns. After ſome labour we arrived at the top of the preci-pice. I found myſelf on a very narrow ridge, thickly covered with wood, and bounded by two ravins, the bottoms of which I could not ſee; the deſcent to them ſeemed to be nearly per-pendicular, yet all the way covered with thick wood. After re-freſhing ourſelves, we began our fatigue, the boy and I cutting, and carrying our water and proviſions, alternately. When we had got ſome way, I found I was on an exceeding narrow ridge, in many parts not ſix feet broad; on each ſide a tremendous gulf, into one or other of which I was often in danger of fal-ling, ſo that with great caution I was obliged to lie down on my belly, to ſee through the buſhes how the ridge tended. Here I began to ſmell ſulphur, or rather a ſmell like gunpowder. As I knew this ſmell muſt come from the top of the mountain, being in the direction of the wind, I was in hopes we could not be far from it, as the ſmell grew ſtronger and ſtronger as I aſcended. I ſaw a riſing before me, and thought if I was once on it, if the top of the mountain was near I could have a view of it; but having got on this riſing I could only ſee a high peak on the N.W. end of the mountain, and by appearance I thought

myſelf very little nearer than when I was at the bottom. The woods now became very difficult to get through; great quantities of fallen trees lying buried under long graſs and being rotten, when I thought myſelf walking on the ground, I was frequently buried a great depth among them. Being now about noon, and my turn to carry the baggage, and conſequently my turn of reſt, I was ſurpriſed to hear a ruſtling among the buſhes, and ſomething like a human voice behind me. As we were now in a place where I had little reaſon to ſuppoſe there had been a human foot before, and could not imagine there could be habitations of Caribbees or run-away negroes, ſince from the barrenneſs of the mountain they could not poſſibly find any proviſions to ſubſiſt on, I told the boy to ſtand ſtill, and let us wait their coming up; for if they were Caribbees advancing with an intention to hurt us, there was no alternative but to defend ourſelves. You may imagine my ſurpriſe when I ſaw one of the negroes who had been with me the day before, with three others, which Mr. MALOUNE had ſent to my aſſiſtance, with plenty of proviſions. After refreſhment, with this aſſiſtance, I renewed my labours with freſh ſpirits, and thought I was ſure of reaching the top before night. Having proceeded a little, I had a fair view of the ravin on my left, which was of prodigious depth, and ran from near the top of the mountain to the ſea; its bottom ſeemed to be a rock of a colour nearly reſembling lava, and appeared as if there had been vaſt torrents of ſulphureous matter running in it ſome time. I regretted much I knew not of this ravin before I commenced my excurſion, as by paſſing a head-land in a canoe, and getting into the ravin, I might have gained the ſummit of the mountain, without experiencing the delays and difficulties I here encountered. It was now about

4 P.M.

4 P.M. and I had no proſpect of the mountain's top; but from the aſcent of the ravin below, I knew it was a great way off. I thought if I could get into the ravin before night, I could get eaſily up next morning. After cutting a great way through wild plantains, the ſun near ſetting, I found myſelf almoſt over the verge of a precipice; by catching hold of ſome ſhrubs I prevented myſelf from falling. We were now about half-way down; but all the way below us, as far as we could ſee, was a perpendicular precipice of rock, ſeveral hundred feet high, to paſs which was impoſſible. I had a view of ſome part of the top of the mountain, which I ſaw was yet far from me; nor could I attempt any other way than the ridge I had left. Being now ſun-ſet, and the negroes very diſcontented, becauſe they could not return that night, I found we muſt take up our night's reſidence in the place where we were. It was a very unfavourable one, there being nothing but plantains growing, which retaining the rain long in their leaves, and being frequently agitated by the wind, were conſtantly dropping, and kept the ground always moiſt. Being almoſt dark, we had time to make us no other habitation, than placing two or three ſticks againſt an old ſtump of a tree, and ſlightly covering them with plantain leaves. After getting together ſome little wood to make a fire to keep us comfortable, it began to blow and rain violently, which continued all night. We ſoon found our building afforded us no ſhelter, and the wood would not burn, ſo that we could not get any fire; and the ground on which we were ſituated would not allow the leaſt exerciſe to keep us warm. From ſuch a miſerable night I experienced no mitigation for the fatigues of the day. I wiſhed for the riſing ſun, to renew my labours; which I at laſt beheld with inexpreſſible joy.

As

As ſoon as we could ſee, we returned to the ridge we left the night before, and began to work with alacrity, as we were almoſt chilled with cold. I puſhed on as faſt as poſſible, and about ten o'clock found the woods began to grow thin. I could not ſee the top of the mountain, but had a view of ſeveral ridges that joined it. From the wind falling, and the heat growing intenſe, I thought we muſt then be under the cover of the ſummit: I here found many new plants. About eleven A.M. I was overjoyed to have a full view of the ſummit of the mountain, nearly a mile diſtant from us, and that we were nearly out of the woody region. The top ſeemed to be compoſed of ſix or ſeven different ridges, very much broken in the ſides, as if they had ſuffered great convulſions of nature; they were divided by amazing deep ravins. without any water in them. I obſerved where the ridges meet the edge of a large excavation, as it ſeemed to be, on the higheſt part. I imagined this might be the mouth of the crater, and directed my courſe to a high peak which overlooked it. I found here a moſt beautiful tree which compoſed the laſt wood. After that I entered into a thick long graſs, intermixed with fern, which branched and ran in every direction. To break it was impoſſible, and with great difficulty I could cut it; ſo that in clearing our way through this graſs, eight or ten feet high, there was equal difficulty as in the woods, and it ſeemed to continue very near to the top of the mountain. Being now about noon, I and the negroes were ſo fatigued as hardly to be able to ſtand; our thirſt very great, to allay which, as much as poſſible, we chewed the leaves of the Begonia obliqua. Two of the negroes returned, and the others ſaid they would go no farther with me, as they muſt periſh for want of water, and it would be impoſſible to get to the bottom before night, and they muſt all

die in the woods. The propriety of their reaſoning was evident to me; yet I thought it hard, after the fatigues of three days and two nights, to be within half a mile of the top, and not be able to get up, and to know little more about it than I did at the bottom. As the negroes had not the ſame motive for going up as I, all my reaſoning was to them ineffectual; I found I was obliged to return myſelf, as I could not perſiſt alone. At half paſt twelve we began to deſcend the ſame way we came. As there was now a clear path all the way to the bottom, we got down to Mr. GASCO's by ſun ſet. After ſitting ſome time here, I was hardly able to riſe again, I was ſo tired; and my feet were ſo ſore I could hardly ſtand on them, for, my ſhoes being torn to pieces, I came down the whole way bare-footed. I continued my journey, however, to Mr. MALOUNE's, where I arrived between ſix and ſeven at night.

March 4th, being the day I had fixed to finiſh my excurſion, about four in the morning, I left the houſe of Mr. FRASER, who out of curioſity agreed to accompany me, of which I was very glad, as he was a ſenſible young man; and with the aſſiſtance of two negroes we purſued our journey. We found very little obſtruction in our way up, until we got to the place where I returned; and there, for about a quarter of a mile, we had conſiderable difficulty to clear our way through graſs and ferns. After we came within a quarter of a mile from the top, we found ourſelves in another climate all at once, the air very cold, and the vegetable productions changed; here was nothing but barrenneſs over the whole ſummit of the mountain. On the confines of the graſſy region and the barren I found ſome beautiful plants. Moſs grows here in ſuch plenty, that I frequently ſunk up to my knees in it. This is the only place in the Weſt Indies that produced any moſs that I have ſeen. About noon

we gained the top of the peak I had directed my courſe to before; when, in an inſtant, we were ſurpriſed with one of the grandeſt and moſt awful ſcenes I had ever beheld. I was ſtruck with it amazingly, as I could not have conceived ſuch a very large and ſo ſingularly formed an excavation. It is ſituated on the center of the mountain, and where the various ridges unite. Its diameter is ſomething more than a mile, and its circumference to appearance a perfect circle. Its depth from the ſurrounding margin is above a quarter of a mile, and it narrows a little, but very regularly, to the bottom. Its ſides are very ſmooth, and for the moſt part covered with ſhort moſs, except towards the ſouth, where there are a number of ſmall holes and rents. This is the only place where it is poſſible to go down to the bottom: it is exceedingly dangerous, owing to the numberleſs ſmall chaſms. On the weſt ſide is a ſection of red rock like granite, cut very ſmooth, and of the ſame declivity with the other parts. All the reſt of the ſurrounding ſides ſeems to be compoſed of ſand, that looks to have undergone the action of intenſe fire. It has a cruſt quite ſmooth, of about an inch thick, and hard almoſt as rock; after breaking through which, you find nothing but looſe ſand. In the center of the bottom is a burning mountain of about a mile in circumference, of a conic form, but quite level. On the ſummit, out of the center of the top, ariſes another mount, eight or ten feet high, a perfect cone; from its apex iſſues a column of ſmoke. It is compoſed of large maſſes of red *granite-like* rock of various ſizes and ſhapes, which appear to have been ſplit into their preſent magnitudes by ſome terrible convulſion of nature, and are piled up very regular. From moſt parts of the mountain iſſue great quantities of ſmoke, eſpecially on the north ſide, which appears to be burning from top to bottom,

and the heat is ſo intenſe, that it is impoſſible to go upon it. Going round the baſe is very dangerous, as large maſſes of rock are conſtantly ſplitting with the heat, and tumbling to the bottom. At the bottom, on the north ſide, is a very large rock ſplit in two; each of theſe halves, which are ſeparated to a conſiderable diſtance from each other, is rent in all directions, and from the crevices iſſue efflorescences of a gloſſy appearance, which taſte like vitriol, and alſo beautiful cryſtallizations of ſulphur. On all parts of the mountain are great quantities of ſulphur in all ſtates; alſo alum, vitriol, and other minerals. From the external appearance of this mountain, I imagine it has only begun to burn lately, as on ſeveral parts of it I ſaw ſmall ſhrubs and graſs, which looked as if they had been lately ſcorched and burnt. There are ſeveral holes on the ſouth, from which iſſues ſmoke, ſeemingly broken out lately, as the buſhes round are but lately burnt. On two oppoſite ſides of the burning mountain, eaſt and weſt, reaching from its baſe to that of the ſide of the crater, are two lakes of water, about a ſtone's throw in breadth; they appear to be deep in the middle; their bottom to be covered with a clay-like ſubſtance. The water ſeems pleaſant to the taſte, and is of a chalybeate nature. I ſuppoſe theſe lakes receive great increaſe, if they are not entirely ſupported, by the rain that tumbles down the ſide of the crater. I obſerved on the north ſide of the bottom traces of beds of rivers, that to appearance run great quantities of water at times to both theſe lakes. By the ſtones at their edges, I could perceive that either abſorption or evaporation, or perhaps both, go on faſt. The greater part of the bottom of the crater, except the mountain and two lakes, is very level. On the ſouth part are ſeveral ſhrubs and ſmall trees. There are many ſtones in it that ſeem to be impregnated with minerals: I ſaw ſeveral pieces of pu-

mice-ſtone. I alſo found many ſtones about the ſize of a man's fiſt, rough, on one ſide blue, which appearance, I imagine, they have got from heat, and being in contact with ſome mineral. Theſe ſtones are ſcattered over the whole mountain, one or two of which I have ſent you, with ſome others.

After I had got up from the bottom of the crater, I could not help viewing it with admiration, from its wonderful ſtructure and regularity. Here I found an excavation cut through the mountain and rocks to an amazing depth, and with as much regularity and proportion of its conſtituent parts, as if it had been planned by the hand of the moſt ſkilful mathematician. I wiſhed much to remain on the mountain all night, to examine its ſeveral ridges with more attention next day; but I could not prevail on my companion to ſtay, and therefore thought it adviſable to accompany him.

I obſerved the motion of the clouds on this mountain to be very ſingular. Although there are ſeveral parts on it higher than the mouth of the crater, yet I ſaw their attraction was always to it. After entering on its eaſt or windward ſide, they ſunk a conſiderable way into it; then, mounting the oppoſite ſide, and whirling round the north-weſt ſide, they ran along a ridge, which tended nearly north-eaſt, and afterwards ſunk into a deep ravin, which divided this ridge from another on the north-weſt corner of the mountain, and the higheſt on it, lying in a direction nearly ſouth and north. They keep the courſe of this ridge to the ſouth end, and then whirl off weſt in their natural courſe.

I took my departure from the mountain with great reluctance. Although I encountered many difficulties to get up, yet it amply rewarded me for all my toil; but I had not time to examine it with that attention I wiſhed. When I got on the

peak

peak from which I had my firſt view of it, and from which I could ſee its different parts, I could not help reviewing it ſeveral times. After imprinting its ſtructure on my mind, I took my final adieu of it, and returned down, and got to Mr. FRASER's houſe about ſeven at night, much fatigued.

I am ſorry I had no inſtruments, to take the ſtate of the air, nor the exact dimenſions of the different parts of the mountain; but, I believe, on meaſurement, they will be more than I have mentioned.

From the ſituation of theſe iſlands to one another, and to the continent of South America, I imagine there are ſub-marine communications between the burning mountains or volcanoes in each of them, and from them to the volcanoes on the high mountains of America. The iſlands, which are ſituated next the continent, ſeem to tend in the direction of thoſe mountains; and I have obſerved, that the crater in this iſland lies nearly in a line with Soufriere in St. Lucia and Morne Pelée in Martinique, and I dare ſay from Morne Pelée to a place of the ſame kind in Dominique, and from it to the others; as it is certain there is ſomething of this kind in each of theſe iſlands, Barbadoes and Tobago excepted, which are quite out of the range of the reſt.

There is no doubt but eruptions or different changes in ſome of them, although at a great diſtance, may be communicated to and affect the others in various manners. It is obſerved by the inhabitants round theſe burning mountains, that ſhocks of earthquakes are frequent near them, and more ſenſibly felt than in other parts of the iſland, and the ſhocks always go in the direction of them.

I cannot omit mentioning the great aſſiſtance I received in the above excurſion from Dr. YOUNG, Mr. MALOUNE, and Mr. FRASER;

FRASER; for, without the aid of their negroes, I could not have poſſibly gone through with it.

References to the figure, tab. II.

A 1. The ſummit that overlooks the crater, from which the drawing is taken.

AAAA. The circumference of the crater.

BBBB. The circumference of the bottom.

C. The burning mountain.

D. The ſmall one on its ſummit.

EE. The two lakes of water.

F. The ſection of the rock on the weſt ſide of the crater.

G. The large ravin.

HHHH. Ravins of great depth.

I. Efflorescence on the north end of the rock, which at a diſtance looks like alum or nitre.

1.2.3.4.5.6. The different ridges on the ſummit of the mountain, as they join the crater.

7. Woods deſtroyed by the hurricane.

8.8. The clouds going to the ſouthward of the weſt ridge, after paſſing north on the weſt ſide of the crater.

9.9.9. Where I deſcended into the bottom of the crater.

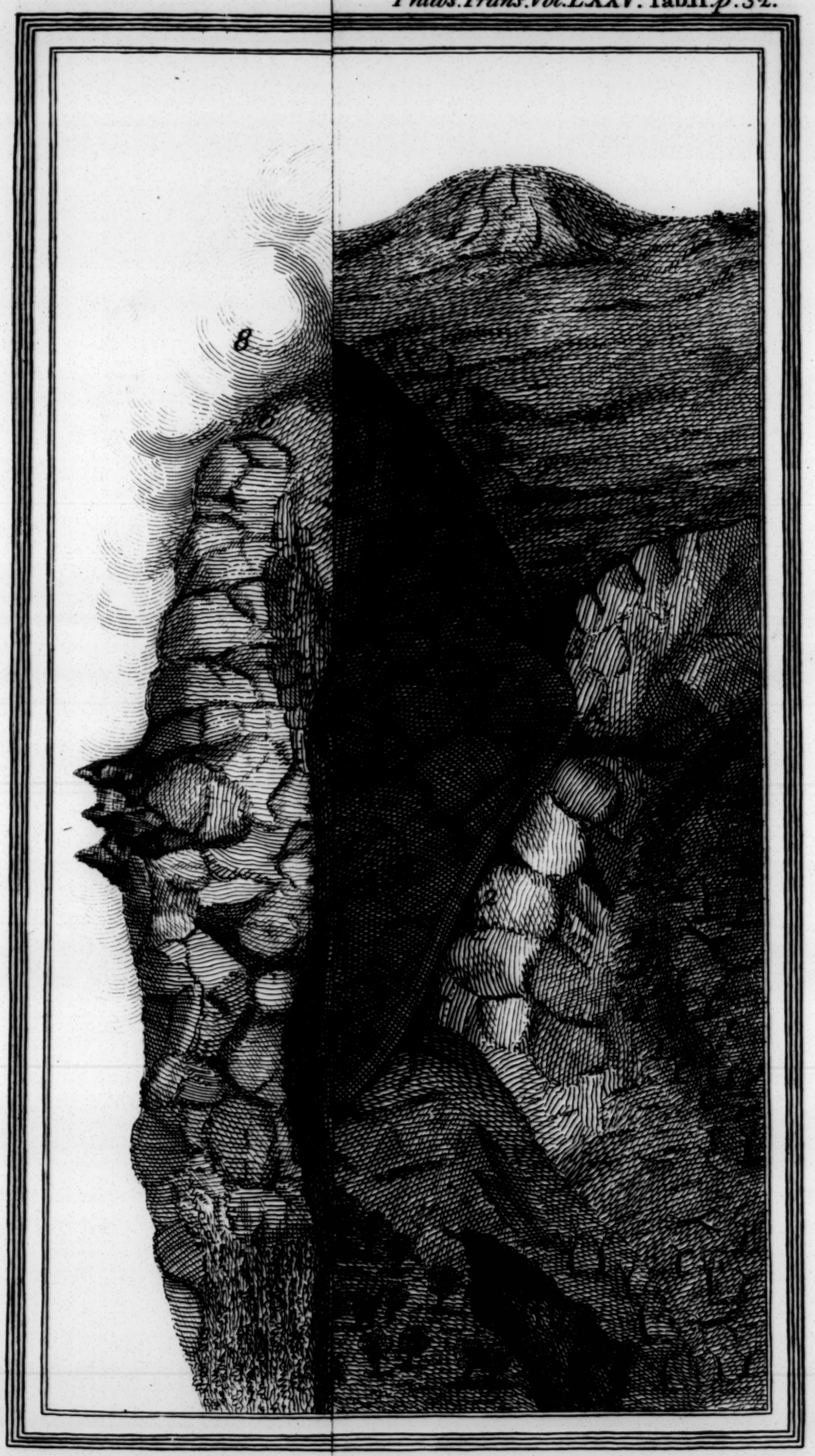
1 and 10. The ſummit and baſe of the ridge on which I aſcended the mountain.

IV. *A Supplement to the Third Part of the Paper on the Summation of infinite Series, in the* Philoſophical Tranſactions *for the Year* 1782. *By the Rev.* S. Vince, *M. A.; communicated by* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read November 25, 1784.

THE reaſoning in the third part of my paper on the Summation of infinite Series having been miſunderſtood, I have thought it proper to offer to the Royal Society the following explanation. When I propoſed, for example, to ſum the ſeries $\frac{1}{2} - \frac{2}{3} + \frac{3}{4} -$ &c. *ſine fine*, I wanted to find ſome quantity which, by its expanſion, would produce that ſeries, and that quantity I called its ſum; not (as I conceived muſt have been evident to every one) in the common acceptation of that word, that the more terms we take, the more nearly we ſhould approach to that quantity, and at laſt arrive nearer to it than by any aſſignable difference, for there manifeſtly can be no ſuch quantity; but as being a quantity from which the ſeries muſt have been deduced by expanſion, which quantity I found to be $-\frac{1}{2} +$ H. L. 2. If therefore in the ſolution of any problem, the concluſion, whoſe value I want, is expreſſed by the above ſeries, and which aroſe from the neceſſity of expanding ſome quantity in the preceding part of the operation, ſurely no one can deny but that I may ſubſtitute for it $-\frac{1}{2} +$ H. L. 2. For whatever quantity it was, which by its expanſion produced at

firſt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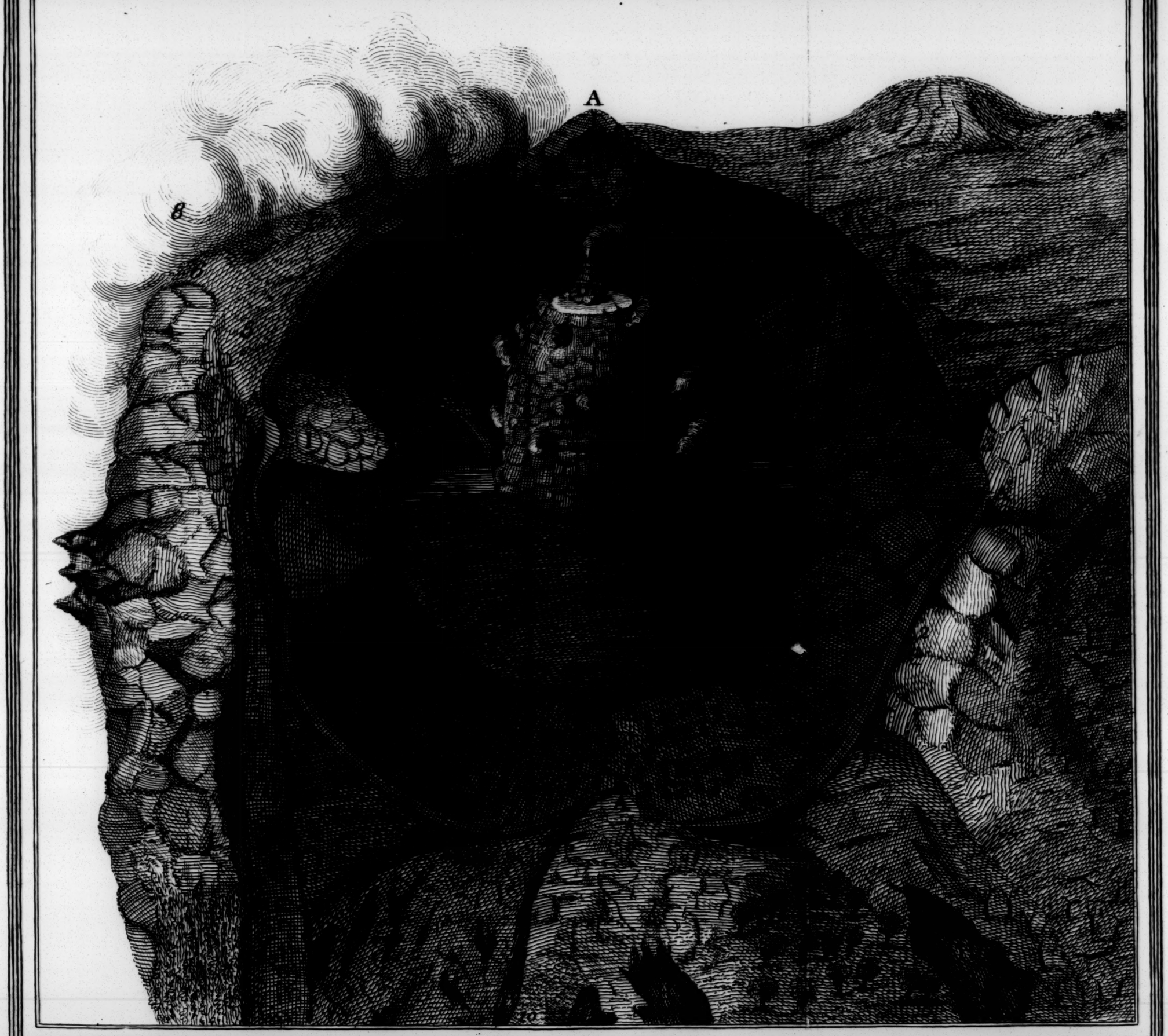
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firſt a ſeries, the ſame reduction which, from that ſeries, produced the ſeries $\frac{1}{2} - \frac{2}{3} + \frac{3}{4} -$ &c. muſt alſo have produced $-\frac{1}{2} +$ H. L. 2. from the quantity which was expanded. This value of the ſeries I obtained in the following manner. I ſuppoſed the ſeries $\frac{1}{2} - \frac{2}{3} + \frac{3}{4} -$ &c. to be divided into two parts; the firſt part to contain all the terms till we come to thoſe where the numerators and denominators become both infinitely great, in which caſe every term afterwards may be ſuppoſed to be equal to unity: the ſecond part, therefore, would neceſſarily be (ſuppoſing the firſt part to terminate at an even number of terms) $1 - 1 + 1 - 1 +$ &c. *ſine fine*. The firſt part, by collecting two terms into one, becomes $-\frac{1}{2 \cdot 3} - \frac{1}{4 \cdot 5} - \frac{1}{6 \cdot 7} -$ &c. which ſeries, as it is continued till the terms become infinitely ſmall, is equal to $-1 +$ H. L. 2. The ſecond part $1 - 1 + 1 -$ &c. has not, taken abſtractedly of its origin, any determinate value (as will be afterwards obſerved), but conſidered as part of the original ſeries it has, for that ſeries muſt have been deduced from the expanſion of the binomial $\overline{1+x}^{-1}$, or $\frac{1}{1+x}$; and hence, when $x = 1$, $1 - 1 + 1 -$ &c. can in this caſe have come only from $\frac{1}{1+1}$, which, therefore, muſt be ſubſtituted for it; conſequently the two parts together give $-\frac{1}{2} +$ H. L. 2.

Having thus explained the nature of the ſeries which I propoſed to ſum, and the principle upon which the correction depends, I muſt beg leave to acknowledge my obligations to my very worthy and ingenious friend GEORGE ATWOOD, Eſq. F.R.S. who firſt obſerved that the ſeries $1 - 1 + 1 - 1 +$ &c. has no determinate value in the abſtract, as it may be produced by $\frac{1}{1+1+1+\text{&c.}}$ whatever be the number of units in the denomi-

nator *; and it may alſo be added, that the ſame ſeries ariſes from $\frac{1+1+1+\&c.}{1+1+1+1+\&c.}$, provided the number of units be greater in the denominator than in the numerator. The correction will therefore be different in different circumſtances, and will depend on the nature of the quantity which was at firſt expanded. In the third part of my paper, I applied the correction to thoſe caſes where the original ſeries aroſe from the expanſion of a binomial, where the correction is in general as I there gave it; but as I did not apply my method to any other ſeries, I confeſs that it did not appear to me, that the correction would then be different, which it neceſſarily would had I extended my reaſoning to other caſes. I ſhall therefore add one example to ſhew the method of correction in other inſtances, where the value of the correction will be found to be different, according as we begin to collect at the firſt or ſecond term. Let the ſeries be $\frac{2}{1}-\frac{3}{2}+\frac{5}{4}-\frac{6}{5}+\frac{8}{7}-\&c.$ *ſine fine*, which came originally from $\frac{1}{1+x+x^2}$; now if we begin to collect at the firſt term, the ſeries becomes $\frac{1}{1\cdot 2}+\frac{1}{4\cdot 5}+\&c.$ and for the ſame reaſon as before, the correction, to be added, is $\frac{1}{3}$; but $\frac{1}{1\cdot 2}+\frac{1}{4\cdot 5}+\&c. = \frac{4}{3}$ of a circular arc (A) of 30° to the radius $\frac{\sqrt{3}}{2}$; hence the ſum required $= \frac{4}{3}A+\frac{1}{3}$. If we begin to collect at the ſecond term the ſeries becomes $2-\frac{2}{2\cdot 4}-\frac{2}{5\cdot 7}-\&c.$; and the correction to be ſubtracted is $\frac{2}{3}$; for the ſecond part of the original ſeries is now $-1+1-1+1-\&c.$ which was produced by $\frac{1+1}{1+1+1}$; but

* I have been ſince informed by Mr. WALES, F. R. S. that a pupil of his, Mr. POND, made the ſame obſervation.

$2 - \frac{2}{2 \cdot 4} - \frac{2}{5 \cdot 7}$ - &c. $= 1 + \frac{4}{3}$ A; therefore the ſum required $=$ $\frac{1}{3} + \frac{4}{3}$ A as before. In the ſame manner we may apply the correction in all other caſes. Although, therefore, the ſeries $1 - 1 + 1 - 1 +$ &c. or $-1 + 1 - 1 + 1 -$ &c. have no determinate value in the abſtract, yet the given ſeries will fix its value by pointing out the quantity from which the ſeries muſt have been originally produced.

V. *Deſcription of a Plant yielding* Aſa fœtida. *In a Letter from* John Hope, *M. D. F. R. S. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read December 9, 1784.

TO SIR JOSEPH BANKS, BART. P. R. S.

SIR, Edinburgh, Auguſt 18, 1784.

I BEG you will do me the honour of preſenting the incloſed account of the Aſa fœtida, and the botanical deſcription of the plant, with the drawings, to the Royal Society.

I have the honour of being, with much reſpect and eſteem, &c.

JOHN HOPE.

ASA FOETIDA.

PLANTA umbellifera, tripedalis, erecta, ramoſa, glauca, flore luteo.

Radix perennis.

Folia radicalia ſex, procumbentia, trilobo-ovata, multoties pinnatim diviſa; foliolis inciſis, ſubacutis, ſubdecurrentibus; petiolo communi ſuperne plano, linea elevata longitudinaliter per medium decurrente.

Caulis bipedalis, erectus, teretiuſculus, annuus, leviter ſtriatus, glaber, nudus præter unam circa medium foliorum imperfectorum conjugationem ; petiolo membranaceo, concavo.

Rami nudi, patuli; quorum tres inferi, alterni, ſuſtinentur ſinguli folii imperfecti petiolo membranaceo concavo.

Quatuor intermedii verticillati ſunt. Supremi ex apice caulis octo, quorum interni erecti.

Omnes hi rami ſummitate ſuſtinent umbellam compoſitam ſeſſilem terminalem, et præterea 3—6 ramulos externe poſitos, umbellas compoſitas ferentes.

Hoc modo, rami inferiores ſuſtinent 5, raro 6 ramulos ; intermedii 3 vel 4; ſuperiores 1 et 2.

CAL. *Umbella univerſalis* radiis 20—30 conſtat.

——— *partialis* floſculis ſubſeſſilibus 10—20.

Umbella compoſita ſeſſilis convexo-plana.

——— ——— pedunculata hæmiſpherica.

Involucrum univerſale nullum.

——— *partiale* nullum.

Perianthium proprium vix notabile.

COR. *univerſalis* uniformis.

Floſculi umbellæ ſeſſilis fertiles.

——— ——— pedunculatæ plerumque abortiunt.

propria petalis quinque æqualibus, planis, ovatis : primo patulis, dein reflexis, apice aſcendente.

STAM. *Filamenta* 5, ſubulata, corolla longiora, incurvata. *Antheræ* ſubrotundæ.

PIST. *Germen* turbinatum, inferum.

Styli duo, reflexi.

Stigmata apice incraſſata.

PER.

PER. nullum: fructus oblongus, plano-compreſſus, utrinque 3 lineis elevatis notatus eſt.

SEM. duo, oblonga, magna, utrinque plana, 3 lineis elevatis notata.

Planta odorem alliaceum diffundit. Folia, rami, pedunculi, radix, truncus, ſecti ſuccum fundunt lacteum, ſapore et odore Aſæ fœtidæ.

THOUGH Aſa fœtida has been uſed in medicine for many ages, having been introduced by the Arabian phyſicians near a thouſand years ago; yet there was no ſatisfactory account of the plant which yielded it, till KÆMPFER publiſhed his Amœnitates Exoticæ about ſeventy years ago.

KÆMPFER, towards the end of the laſt century, travelled over a great part of Aſia, and was in Perſia, and upon the ſpot where the Aſa fœtida is collected. He gives a full account of the manner of collecting it. He deſcribes the plant; and alſo gives a figure of it, differing in many reſpects from thoſe which I now preſent to the Society *.

Six years ago, I received from Dr. GUTHRIE, of St. Peterſburg, F. R. S. two roots of the Aſa fœtida, with the following card from Dr. PALLAS, addreſſed to Dr. GUTHRIE:

"Dr. PALLAS's compliments to Dr. GUTHRIE; he ſends "him two roots of the Ferula Aſa fœtida, a plant which he

* Probably KÆMPFER's Aſa fœtida Plant is a different ſpecies from that deſcribed by Dr. HOPE in this paper. KÆMPFER was himſelf upon the mountains where the drug is collected, and his fidelity in deſcribing, as well as delineating, has not hitherto been impeached. Sanguis Draconis, and ſome other gums, are indifferently the produce of various ſpecies of plants; and why may not Aſa fœtida be ſimilarly circumſtanced? JOS. BANKS.

"thinks

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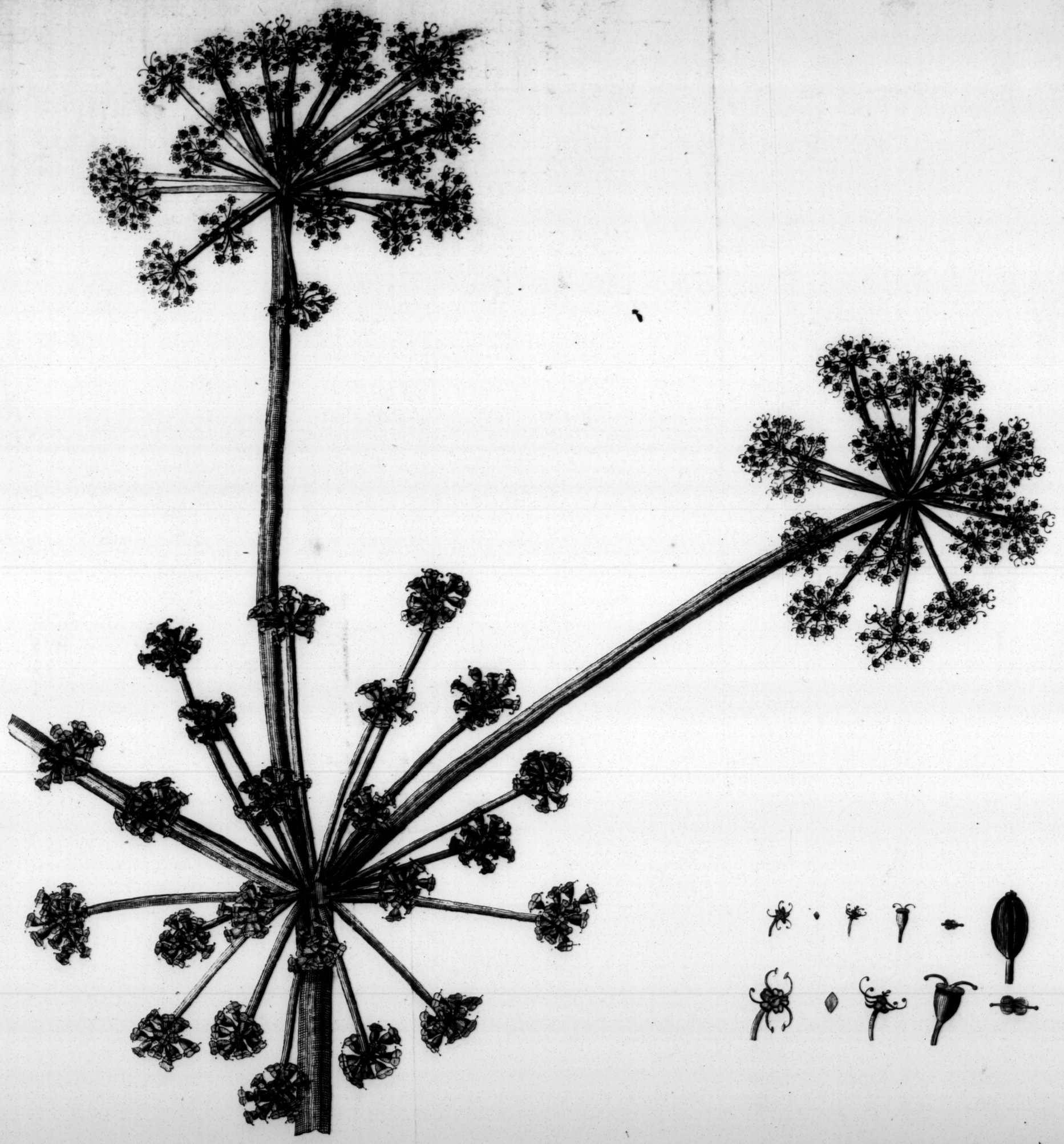

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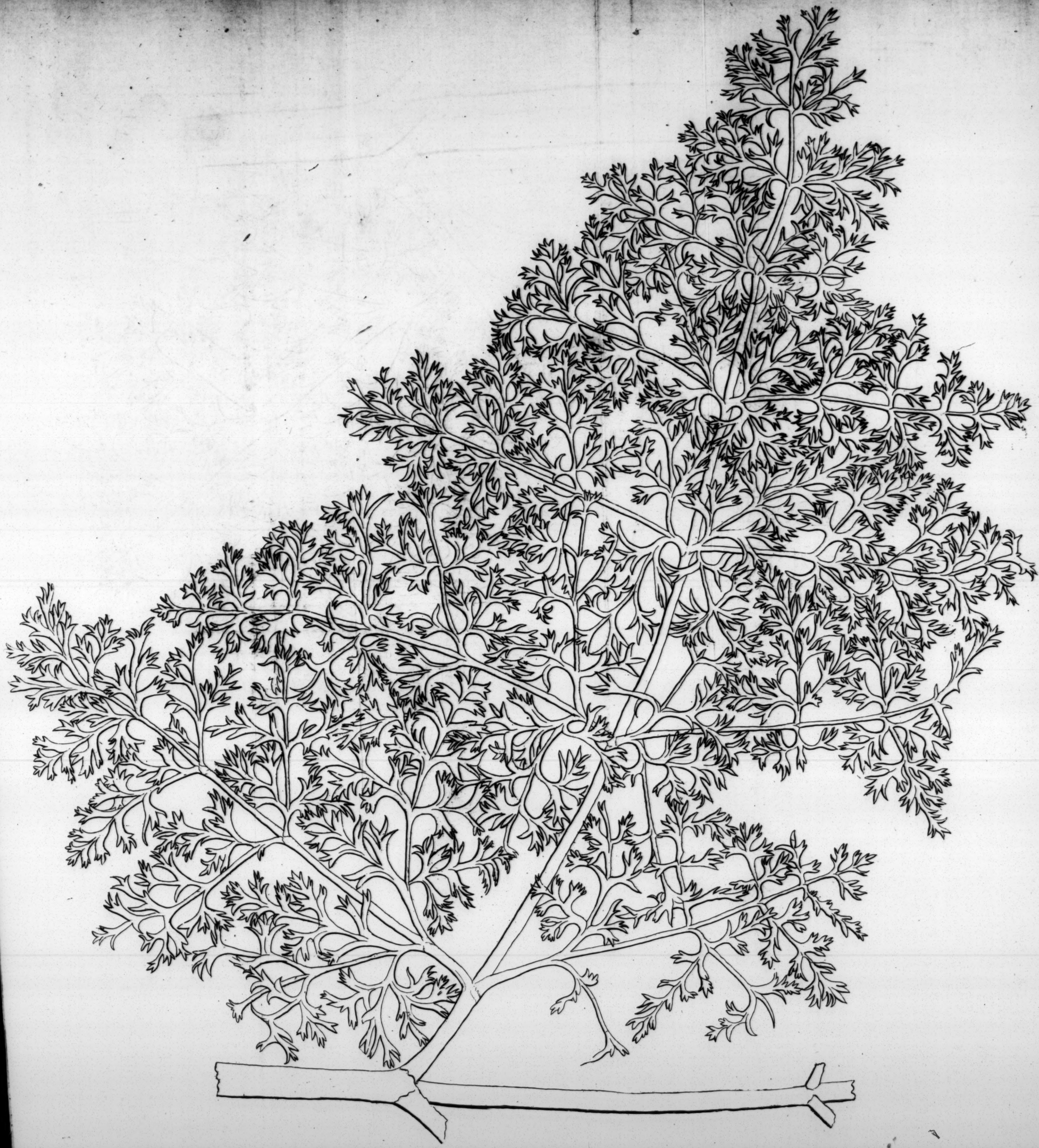

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" thinks never was cultivated in any European garden, and " which nobody has been ſo fortunate as to raiſe from ſeed but " himſelf, though the ſeeds ſent to the Academy from the " mountains of Ghilan in Perſia had been diſtributed among " ſeveral curious perſons."

Both theſe roots were planted in the open ground, in the Botanic Garden at Edinburgh; one died; the other after ſome time did well, and laſt ſummer flowered and produced ſeed. I had an accurate drawing of the plant made by Mr. FIFE, which I now have the pleaſure of laying before the Society. It expreſſes very well the general habit of the plant, which was of a pale ſea-green colour, and grew to the height of three feet. The ſtem is deciduous, but the root is perennial. Every part of the plant, when wounded, poured out a rich milky juice, reſembling in ſmell and taſte Aſa fœtida; and at times a ſmell reſembling garlick, ſuch as a faint impregnation of Aſa fœtida yields, was perceivable at the diſtance of ſeveral feet.

In Perſia, at the proper ſeaſon, the root is cut over once and again; from the inciſions there flows a thick juice like cream, which, thickened, is the Aſa fœtida.

I have only further to obſerve, that as the plant grows in the open air, without protection, and even in an unfavourable ſeaſon produced a good deal of ſeed, and as the juice ſeems to be of the ſame nature with the officinal Aſa fœtida, there is ſome reaſon to hope, that it may become an article of cultivation in this country of no inconſiderable importance.

Edinburgh, Jan. 1783.

VI. *Catalogue of Double Stars.*

By William Herſchel, *Eſq. F. R. S.*

Read December 9, 1784.

INTRODUCTORY REMARKS.

THE great uſe of Double Stars having been already pointed out in a former paper, on the Parallax of the Fixed Stars, and in a latter one, on the Motion of the Solar Syſtem, I have now drawn up a ſecond collection of 434 more, which I have found out ſince the firſt was delivered.

The happy opportunity of giving all my time to the purſuit of aſtronomy, which it has pleaſed the Royal Patron of this Society to furniſh me with, has put it in my power to make the preſent collection much more perfect than the former; almoſt every double ſtar in it having the diſtance and poſition of its two ſtars meaſured by proper micrometers; and the obſervations have been much oftener repeated.

The method of claſſing them is in every reſpect the ſame as that which has been uſed in the firſt collection; for which reaſon I refer to the introductory remarks that have been given with that collection * for an explanation of ſeveral particulars neceſſary to be previouſly known. The numbers of the ſtars are here alſo continued, ſo that the firſt claſs ending there at

* See Philoſophical Tranſactions, vol. LXXII. p. 112.

24 begins here at 25, and the ſame is done with the other claſſes.

Moſt of the double ſtars in my firſt collection are among the number of thoſe ſtars which have their places determined in Mr. FLAMSTEED's extenſive catalogue; but of this collection many are not contained in that author's work, I have therefore adopted a method of pointing them out, which it will be proper to deſcribe.

The finder of my reflector is limited, by a proper diaphragm, to a natural field of two degrees of a great circle in diameter. The interſection of the croſs wires, in the center of it, points out one degree; and by the eye this degree, or the diſtance from the center to the circumference, may be divided into $\frac{1}{4}$, $\frac{1}{2}$, $\frac{3}{4}$, $\frac{1}{3}$, and $\frac{2}{3}$. Thus we are furniſhed with a meaſure which, though coarſe, is however ſufficiently accurate for the purpoſe here intended; and which, if more than two degrees are wanted, may be repeated at pleaſure.

In ſuch meaſures as theſe I have given the diſtance of a double ſtar, whoſe place I wanted to point out, from the neareſt ſtar in FLAMSTEED's Catalogue. And ſince, beſides the diſtance, it is alſo required to have its poſition with regard to the ſtar thus referred to, I have uſed the neighbouring ſtars for the purpoſe of pointing it out.

The uſefulneſs of this method is ſo extenſive, that I ſhall be a little more particular in deſcribing its application. When a ſtar is thus pointed out, as for inſtance the 32d in the firſt claſs, where it is ſaid, "About $\frac{3}{4}$ degree f. preceding the 44th Lyncis, "in a line parallel to θ Urſæ majoris and the 39th Lyncis;" we are to apply one eye to the finder, and placing the 44th Lyncis into the center of the field, we are to look at θ Urſæ majoris and the 39th Lyncis in the heavens with the other eye by the

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ſide of the finder. The naked eye then will immediately direct us, by means of the two ſtars juſt mentioned, towards the place where, in the finder, the armed eye will perceive the double ſtar in queſtion about ¾ degree from the 44th Lyncis. I need hardly obſerve, that we muſt recollect the inverſion of the finder, as thoſe who are in the habit of uſing teleſcopes with high powers, always furniſhed with inverting finders, will of courſe look for the ſmall ſtar in the upper part of the field, as in fig. 1.

At the 45th ſtar, in the firſt claſs, the deſcription ſays, "About 1¼ degree ſ. preceding μ, towards ι Aurigæ." This double ſtar will accordingly be found by placing μ Aurigæ firſt into the center of the finder; then, drawing the teleſcope towards ι, which the naked eye points out, the ſtar we look for will begin to appear in the circumference as ſoon as μ is about ¼ degree removed from the center, as in fig. 2.

It will ſometimes happen, that other ſtars are very near thoſe which are thus pointed out, that might be miſtaken for them. In ſuch caſes an additional precaution has been uſed by mentioning ſome circumſtance either of magnitude or ſituation, to diſtinguiſh the intended ſtar from the reſt. After all, if any obſerver ſhould be ſtill at a loſs to find theſe ſtars without having their right aſcenſion and declination, he may furniſh himſelf with them by means of FLAMSTEED's Atlas Cœleſtis; for my deſcription will be ſufficiently exact for him to make a point in the maps to denote the ſtar's place; then, by means of the graduated margin, he will have its AR and declination to the time of the Atlas, which he may reduce to any other period by the uſual computations.

Before I quit this ſubject I muſt remark, that it will be found on trial, that this method of pointing out a double ſtar is not only

only equal, but indeed ſuperior, to having its right aſcenſion and declination given: for, ſince it is to be viewed with very high powers, not ſuch as fixed inſtruments are generally furniſhed with, the given right aſcenſion and declination would be of no ſervice. We might, indeed, find the ſtar by a fixed or equatorial inſtrument; and, taking notice of its ſituation with regard to other neighbouring ſtars, find, and view it afterwards, by a more powerful teleſcope; but this will nearly amount to the very ſame way which here is purſued, with more deliberate accuracy than we are apt to uſe, while we are employed in ſeeking out an object to look at.

It will be required, that the obſerver ſhould be furniſhed with FLAMSTEED's Atlas Cœleſtis, which muſt have the ſtars marked from the author's catalogue, by a number eaſily added to every ſtar with pen and ink, as I have done to mine. The catalogue ſhould alſo be numbered by an additional column, after that which contains the magnitudes. I hope in ſome future editions of the Atlas to ſee this method adopted in print, as the advantage of it is very conſiderable, both in referring to the catalogue for the place of a ſtar laid down in the Atlas, and in finding a ſtar in the latter whoſe place is given in the former.

I would recommend a precaution to thoſe who wiſh to examine the cloſeſt of my double ſtars. It relates to the adjuſtment of the focus. Suppoſing the teleſcope and the obſerver long enough out in the open air to have acquired a ſettled temperature, and the night ſufficiently clear for the purpoſe; let the focus of the inſtrument be re-adjuſted with the utmoſt delicacy upon a ſtar known to be ſingle, of nearly the ſame altitude, magnitude, and colour, as the ſtar which is to be examined, or upon one ſtar above and another below the

ſame. Let the phænomena of the adjuſting ſtar be well attended to; as, whether it be perfectly round and well defined, or affected with little appendages that frequently keep playing about the image of the ſtar, undergoing ſmall alterations while it paſſes through the field, at other times remaining fixed to it during the whole paſſage. Such deceptions may be detected by turning or unſcrewing the object-glaſs or ſpeculum a little in its cell, when thoſe appendages will be obſerved to revolve the ſame way. Being thus acquainted with the imperfections as well as perfections of the inſtrument, and going immediately from the adjuſting ſtar, which for that reaſon alſo ſhould be as near as may be, to the double ſtar which is to be examined, we may hope to be ſucceſsful. The aſtronomical Mr. AUBERT, who did me the honour to follow this method with γ Leonis, which he did not find to be double when the teleſcope was adjuſted by γ itſelf, ſoon perceived the ſmall ſtar after he had adjuſted it upon Regulus. The inſtrument, being one of Mr. DOLLOND's beſt $3\frac{1}{2}$ feet achromatics, ſhewed Mr. AUBERT the two ſtars of γ Leonis in very cloſe conjunction, or rather one partly hid behind the other. On comparing theſe appearances with my obſervations of that double ſtar, we muſt not be ſurpriſed to find that I place them at a viſible diſtance from each other: for the Newtonian reflectors, on the plan of my 7-feet one, as I have found, will give a much ſmaller image of the ſtars than the $3\frac{1}{2}$ feet achromatic refractors; wherefore the two ſtars, which in refractors as it were run into each other, will in the reflector remain ſeparate. For this reaſon alſo, thoſe who only uſe ſuch refractors muſt not be diſappointed if they cannot perceive the 26th, 30, 31, 36, 41, 44, 46, 47, 60, 75, 82, 86, and 87th ſtars of my firſt claſs to be double,

All the obſervations in the following catalogue on the relative magnitude, colour, and poſition of the ſtars, are to be underſtood as having been made with a power of 460, unleſs they are marked otherwiſe. This will account for the difference which obſervers may find in the relative magnitude; for ſhould they uſe only a power of about 200, many of the ſmall ſtars that are ſaid to be very unequal and extremely unequal, muſt appear to them perhaps a degree lower in the ſcale, and become extremely and exceſſively unequal: and this will happen, though the quantity of light ſhould be the very ſame which the reflector has that ſerved me to ſettle theſe particulars. I need not ſay, that on other accounts, ſuch as a real difference in the light of the teleſcope, the preſence of the moon, twilights, auroræ boreales, or other cauſes, many of the ſmall ſtars may be found to be of a different comparative luſtre from what is aſſigned to them in the catalogue. The ſmall ſtar near Rigel, for inſtance, appears of a beautiful pale red colour, full, round, and well defined, with my 20-feet reflector; the 10-feet inſtrument ſhews it alſo very well in fine evenings; the 7-feet requires more attention, nor is the ſmall ſtar defined, but of a duſky pale red colour. A good $3\frac{1}{2}$ feet achromatic, of a large aperture, when Rigel is on the meridian, may, perhaps, alſo ſhew the ſmall ſtar, although I have not been able to ſee it with a very good inſtrument of that ſort, which ſhews the ſmall ſtar that accompanies the pole-ſtar; but the evening was not very favourable.

The meaſures of the diſtances were all taken with a parallel ſilk-worm's-thread micrometer, and a power of 227 only. They are not, as in the former catalogue, with the diameters included, but from the center of one ſtar to the center of the other.

other. I have adopted theſe meaſures on finding that I could procure threads fine enough to ſubtend only an angle of about 1″ 13‴, and that by this means there was no longer any great difficulty of judging when the ſtars were centrally covered by the threads. However, I do not know whether theſe meaſures, with ſtars at a conſiderable diſtance, may not be liable to an additional error of perhaps one ſecond, owing to the remaining uncertainty in judging of their exact central poſition while the meaſure is taking.

The poſitions have all been meaſured (unleſs marked otherwiſe) with a power of 460, adapted to an excellent micrometer, executed by Meſſ. NAIRNE and BLUNT, according to the model given in the Philoſophical Tranſactions, vol. LXXI. page 500. fig. IV.; but with a great and neceſſary improvement of making the wheel *d*, *d*, of that figure perform its whole revolution; by which means the two ſilk-worms-threads may be adjuſted to a greater degree of exactneſs; for if they are not placed ſo as perfectly to biſect the circle, the two threads will not coincide exactly after having performed one ſemi-revolution, which they muſt be made to do with the utmoſt rigour. I found the abſolute neceſſity of this precaution when I came critically to examine the poſitions of the Georgium Sidus, as they are given in table III. Phil. Tranſ. vol. LXXI. p. 497. The meaſures were affected with a ſmall and pretty regular error, which I was at a loſs to account for; and the diſtance of this ſtar being then totally unknown, I looked for the cauſe of the deviation at firſt in a diurnal parallax of that heavenly body; but ſoon found it owing to the inconvenience before-mentioned, of not being able experimentally to adjuſt the moveable thread to that critical nicety which I

have

have now introduced and uſed in all the angles of the following catalogue *.

Datchet near Windſor, Nov. 1, 1784. W. HERSCHEL.

CATALOGUE OF DOUBLE STARS.

FIRST CLASS.

I. 25. A Orionis. Fl. 32. Sub humero in conſequentia.
Jan. 20. 1782. Double. Conſiderably unequal. L. fine w.; S. w. inclining to pale roſe colour. The diſtance or black diviſion between the two ſtars with 278 is about ¼ diameter of L.; with 460, near ½ diameter of L. Poſition with 278, 52° 10′ f. preceding.

26. ω Leonis. Fl. 2. Anteriorem pedem dextrum præcedens.
Feb. 8. 1782. A very minute double ſtar. Conſiderably unequal. Both r. With 227 there is not the leaſt ſuſpicion of its being double; with 460 it appears oblong, and, when perfectly diſtinct, we ſee ¾ of the apparent diameter of a ſmall ſtar as it were emerged from behind a larger ſtar; with 932 they are more clear of each other, but not ſeparated; the focus of every power adjuſted upon the 3d and 6th Leonis. November 6th, 1782, I

* The diviſions on the moveable circular index (*a*) of this micrometer ſhould be read off by means of a line drawn on a ſmall plate faſtened to the ſide *t*, and projecting with a proper curvature againſt the plane of the diviſions towards *r*, ſo as to be nearly in contact; a coincidence of lines being by far the beſt method of aſcertaining the ſituation of the index. A nonius of four ſub-diviſions may alſo be uſed, whereby the 60 diviſions, already divided into halves upon the index-plate, will be had in eighths, each of which, on the conſtruction of my preſent one, will be equal to three minutes of a degree of the circle.

I. first ſuſpected a ſeparation; and November 13th, fairly ſaw a diviſion between them. April 4, 1783, with an improved reflector of 20 feet 3 inches focal length and 12 inches aperture, I ſaw them evidently divided. Poſition 20° 54′ ſ. following *.

27. FL. 90 Leonis. Infra eductionem caudæ.

Feb. 9. Treble. The two neareſt—very unequal. L. w.;
1782. S. rw. With 278, 1¼ diameter of L; with 460, 1½ diameter of L. Poſition with 278, 61° 9′ ſ. preceding. The two fartheſt—very unequal. S. duſky r. Diſtance from L. 53″ 43‴. Poſition 35° 12′ ſ. preceding.

28. γ Leonis. FL 41. In collo lucida.

Feb. 11. A beautiful double ſtar. Pretty unequal. L. w.;
1782. S. w. inclining a little to pale red. With 227 and 278 diſtinctly ſeparated; with 460, ⅙ diameter of S.; with 625, ¼ diameter; with 932, full ¼ diameter, or when

* I ſuſpect theſe ſtars to recede from each other. It is, however, very poſſible, that the opening which I obſerved between them, at the latter end of the year 1782 and beginning of 1783, may be owing to very favourable weather, or to my being better acquainted with the object. Could we increaſe our power and diſtinctneſs at pleaſure, we might undoubtedly ſeparate any two ſtars that are not abſolutely in a direct line paſſing through the eye of the obſerver, and the centers of both the ſtars. This will appear when we conſider that perhaps 59 thirds out of one ſecond, which the diameter of the ſtar may ſubtend, are ſpurious; ſo that a double ſtar ſeemingly in contact, or even partly hiding each other in appearance, may ſtill be far enough aſunder to admit of a fair and conſiderable ſeparation by applying an adequate magnifying power. It would have been curious, if a conſiderable difference in the colours could have led us to diſcover which of the two ſtars is before the other! But the far greateſt part of their apparent diameters being, as we have obſerved, ſpurious, it is probable, that a different coloured light of two ſtars would join together, where the rays of one extend into thoſe of the other; and ſo, producing a third colour by the mixture of it, ſtill leave the queſtion undecided.

beſt

1. beſt ½ diameter of S.; with 1504, ¾ diameter, well-defined, and the difference of colours ſtill viſible; with 2176, not quite a diameter of S, pretty well defined, but exceedingly tremulous; with 2589, leſs than 1 diameter; with 3168, ſtill pretty diſtinct, and about ¾ diameter of S: with 4294, more than a diameter of S, but attended with the utmoſt difficulty of managing the motions; with 5489, the interval ſtill ſomewhat larger, and if the object could be kept in the center of the field, the eye might adapt itſelf to the focus, and get the better of the violent aberration; but the edges of the glaſs being of a different focus, the eye is conſtantly diſappointed in its endeavours to define the object; with 6652, I had but a ſingle glimpſe of the ſtar quite diſfigured; however, I aſcribe it chiefly to the foulneſs of the glaſs, which, on account of its ſmallneſs, is extremely difficult to be cleaned; with a 10-feet reflector, 9 inches aperture, power 626, above ½ diameter of S. very diſtinct; with a 20-feet reflector, power 350, too bright an object to be quite diſtinct, though I ſee it very well. Poſition 5° 24′ n. following. A third ſtar preceding. Diſt. 1′51″23‴, pretty accurate for ſo great a diſtance. Poſition 31° 0′ n. preceding. A fourth ſtar preceding the third, and ſomewhat ſmaller.

29. Parvula juxta Fl. 44^{am} Leonis,

Feb. 17. 1782. Double. About 4′ following the 44th Leonis, which being double in the finder, this is the leaſt of the two. Extremely unequal. L. w. S. d. With 227, 1 ⅓ diameter of L.; with 460, 2 diameters of L. Poſition 26° 32′ n. following.

I. 30. Secunda ad ι Cancri. FL. 57.

March 5. 1782. Double. Pretty unequal. Both pr. With 227, about ¼ diameter; with 278, ¼ diameter; with 460, about ½ diameter or leſs. Poſition 68° 12′ n. preceding. A beautiful minute object.

31. Inter FL. 41^{am} et 39^{am} Lyncis.

March 5. 1782. Double. Near 1¼ degree n. preceding the 41ſt Lyncis; towards *n* Urſæ majoris. A little unequal. Both w. With 460, ¼ or at moſt ⅓ diameter, Poſition 51° 21′ ſ. preceding.

32. FL. 44^{a} Lyncis auſtralior et præcedens.

April 3. 1782. Double. About ¾ degree ſ. preceding the 44th Lyncis; in a line parallel to θ Urſæ majoris and the 39th Lyncis. Very unequal. L. r.; S. bluiſh r. With 227, 1 diameter of L. or 1¼ when beſt; with 460, 1¼ diameter, or when beſt, near 2 diameters of L. The diameters are ſo ſmall that the length of the time, and attention of looking, makes a conſiderable difference in the eſtimation of the diſtance. Poſition 8° 27′ ſ. preceding.

33. ξ Libræ. FL. 51. Primam chelam Scorpii attingens.

May 12. 1782. Treble. Without great attention, and a conſiderable power, it may be miſtaken for a double ſtar; but the largeſt of them conſiſts of two. Very little unequal. Both w. With 460, ¼ or at moſt ⅓ diameter aſunder; with 932, full ⅓ diameter of L. or near ½ diameter of S. Poſition, with 278, 82° 2′ n. following. For meaſures of the third ſtar ſee the 20th of the ſecond claſs.

34. FL. 55. Caſſiopeiæ. ι Ptolemæi. In pedis extremitate.

Treble

I. Treble. The two neareſt very unequal. L. w.; S. colour of pale red blotting paper. With 278, ½ diameter of S. Poſition with 227, 20° 30′ n. preceding. For meaſures of the third ſtar ſee the fourth in the third claſs.

June 11, 1782.

35. Fl. 38. Serpentarii. Dextrum infra pedem.

June 11, 1782. Double. Very unequal. L. w.; S. d. With 460, 1¼ diameter of L. As the ſituation is too low for 460, I tried 227, but it only ſhewed the ſtar wedge-formed. Poſition 60° 48′ n. preceding.

36. ζ Herculis. Fl. 40. In dextro latere.

July 18, 1782. A fine double ſtar. Very unequal. L. w.; S. aſh-colour. With 460, leſs than ½ diameter of S.; with 932, 1 full diameter of S.*. Poſition with 811, 20° 42′ n. following.

37. φ (Fl. 11ª.) Herculis borealior et ſequens.

July 22, 1782. Double. About ⅓ degree n. following φ; in a line parallel to the 35th and 42d Herculis; the moſt ſouth of two very ſmall teleſcopic ſtars. Conſiderably unequal. Both reddiſh. With 227, they can but juſt be ſeen as two ſtars; with 460, near 1 diameter; with 932, not leſs than 1½ diameter of L. Poſition 59° 48′ f. following.

* The interval between very unequal ſtars, eſtimated in diameters, generally gains more by an increaſe of magnifying power than the apparent diſtance of thoſe which are nearer of a ſize. Inſtances of the former may be found in the firſt claſs, the 1ſt, 7, 29, 35, 37, 39, 53, 59, 63, 64, 72d ſtars; of the latter, the 16th, 28, 33, 45, 46, 73, 81ſt ſtars. However, this only ſeems to take place when there is a difficulty of ſeeing the object well with a low power, which being removed by magnifying more, the diſtance is, as it were, laid open to the view.

I. 38. FL. 18^{am} Perſei præcedens ad boream. In capite.

Aug. 20, 1782. Double. About $\frac{1}{2}$ degree n. preceding the 18th; in a line parallel to σ and τ Perſei; of two ſtars that next to the 18th. A little unequal. Both pr. With 278, a moſt minute and beautiful object; with 460, $\frac{1}{2}$ diameter of either. Poſition with 278, 9° 42′ n. preceding.

39. β (FL. 11^{am}) Caſſiopeiæ præcedens ad auſtrum.

Aug. 25, 1782. Double. About $\frac{3}{4}$ degree ſ. preceding β; in a line parallel to η and α Caſſiopeiæ; the following and largeſt of two very conſiderable ſtars. Very unequal. L. pr.; S. r. With 278, $\frac{1}{4}$ diameter of S.; with 460, $\frac{1}{2}$, or when beſt, $\frac{3}{4}$ diameter of S. Poſition 50° 42′ n. preceding.

40. FL. 25^{am} Caſſiopeiæ præcedens ad boream.

Aug. 28, 1782. Double. About $\frac{1}{2}$ degree n. preceding the 25th; towards α Caſſiopeiæ; the firſt teleſcopic ſtar in that direction. Very unequal. Both r. With 460, $\frac{3}{4}$ diameter of S.; difficult to be ſeen. Poſition 50° 30′ ſ. following.

41. FL. 31^{a} Draconis borealior.

Aug. 29, 1782. A very minute double ſtar. About $\frac{3}{4}$ degree n. of the 31ſt; in a line parallel to γ and ξ Draconis; the moſt ſouth and preceding of two. Conſiderably unequal. Both pr. or r. With 227, they appear only as a lengthened or diſtorted ſtar; with 460, $\frac{1}{4}$ diameter of S.; or in very fine nights $\frac{1}{3}$ diameter of S.; with a new ſpeculum and 500, near $\frac{1}{2}$ diameter when beſt; with 932, $\frac{1}{2}$ diameter. Poſition 84° 21′ n. preceding. Requires every favourable circumſtance to be ſeen double.

I. 42. δ Serpentis. Fl. 13. In primo flexu colli.

Sept. 3, 1782. A beautiful double ſtar. Conſiderably unequal. L. w.; S. greyiſh. With 227, ⅓ diameter of S.; with 278, not quite ½ diameter of S.; with 460, near ⅔ diameter of S.; with 932, near 1 diameter of S.; with 1504, above 1 diameter of S. Poſition 42° 48′ f. preceding,

43. Ad Fl. 48^{am} Draconis.

Sept. 3, 1782. A very minute double ſtar. The moſt north of three, forming an arch; or that which is towards *o* Draconis. Conſiderably unequal. Both pale pink. In fine nights, with 460, it has the ſhape of a wedge; with 932, a fine black diviſion juſt viſible; in a very clear dark night a diviſion may be ſeen with 500, and with 932, it will be about ⅛ diameter. Poſition with 500, 88° 24′ n. preceding.

44. Fl. 4. Aquarii. Supra veſtimentum manus ſiniſtræ.

Sept. 3, 1782. A minute double ſtar. Very unequal. Both pr. With 460, almoſt in contact, or at moſt ⅛ diameter of S. Poſition 81° 30′ n. preceding. A third ſtar of the ſixth claſs in view, n. preceding.

45. μ Aurigæ (Fl. 11^{am}) præcedens ad auſtrum.

Sept. 5, 1782. Double. About 1¼ degree ſ. preceding μ, towards ι Aurigæ; a pretty conſiderable ſtar in a minute teleſcopic conſtellation. A little unequal. Both pr. or r. With 227, ⅓ diameter of S.; with 278, near ½ diameter of S.; with 460, about ½ diameter, or near ⅔ diameter of S. Poſition 47° 33′ ſ. preceding.

46. ν (Fl. 13^{am}) Aquarii ſequens ad boream.

Sept. 7, 1782. Treble. About 1¼ degree n. following ν, in a line parallel to β and α Aquarii; the middle of three that

are

I. are in the ſame direction. The two neareſt very unequal. L. rw.; S. pr. With 460, about 1 diameter of L. or more. Poſition 62° 27′ n. preceding. The two fartheſt very unequal. S. pr. Diſtance with 227, 1′ 22″ 42‴. Poſition 35° 51′ n. following.

47. FL. 29^{am} Capricorni præcedens ad boream.

Sept. 27, 1782. A minute double ſtar. About ¾ degree n. preceding the 29th, in a line parallel to γ and α Capricorni. A little unequal. Appears diſtorted with 227 and 278; nor will 460 ſhew it ſeparated; with 657, two ſtars viſible; 932 confirms it. Difficult to be ſeen diſtinctly on account of its low ſituation. Poſition 84° 48′ n. preceding. 20-feet reflector, 200. Both w.

48. FL. 6^{am} Cephei præcedens. In dextro brachio.

Sept. 27, 1782. A very minute and beautiful double ſtar. Near ¾ degree preceding the 6th towards η Cephei; a pretty conſiderable teleſcopic ſtar. A little unequal. Both pr. Almoſt in contact with 460; with 625, better divided; with 657 ſtill better. Poſition 14° 9′ ſ. preceding.

49. λ Cephei (FL. 22^{am}) ſequens ad boream.

Sept. 27, 1782. Double. About 1¼ degree n. following λ, in a line from ζ through λ Cephei continued. Extremely unequal. Both dw. Cannot be ſeen with 278, except with long attention; with 460, 1½ diameter of L. Poſition 85° 48′ n. following; perhaps a little inaccurate.

50. λ Aquarii (FL. 73^{am}) præcedens.

Sept. 30, 1782. Double. About 2⅓ degrees preceding, and a little ſouth of λ Aquarii; a conſiderable ſtar. Very unequal. L. w.; S. dw. With 278, leſs than 1 diameter of L; with 460, 1¼ diameter of L. Poſition with 227,

I. 41° 12′ n. preceding. The meaſure inaccurate on account of the low power, and probably 3° or 4° too ſmall.

51. Quæ ſequitur ι (Fl. 32am) Cephei.

Sept. 30, 1782. Double. About 2¼ degrees n. following ι, towards γ Cephei; a conſiderable ſtar. A little unequal. Both pr. A pretty object with 227; with 460, 1½ diameter nearly. Poſition 3° 36′ ſ. preceding.

52. Parvula Fl. 25tæ Orionis adjecta.

Oct. 2, 1782. Double. A few minutes n. following the 25th Orionis, in a line parallel to *h* Eridani and ε Orionis. Very unequal. L. aſh w.; S. dw. With 460, 1 diameter of L. Poſition 52° 48′ n. preceding.

53. Parvula Fl. 30mæ Orionis adjecta.

Oct. 2, 1782. Double. About 10′ preceding the 30th, in a line parallel to λ and γ Orionis. Very unequal. L. w.; S. d.; with 460, 1 diameter of L. Poſition 43° 24′ n. following.

54. τ (Fl. 20am) Orionis præcedens. In malleolo ſiniſtri cruris.

Oct. 4, 1782. Double. Near ¾ degree preceding τ, in a line from θ through τ Orionis continued. Very unequal. L. r.; S. dr. With 227, about 1 diameter of L.; with 460, about 2 diameters of L. Poſition 35° 42′ n. preceding; a little inaccurate.

55. Fl. 8am Tauri præcedens ad boream.

Oct. 9. 1782. Double. About 1⅓ degree n. preceding the 8th Tauri, or near 2 degrees ſ. following the 65th Arietis, in a line parallel to the Pleiades and ε Tauri; a ſmall teleſcopic ſtar not eaſily found. A little unequal. L. r.; S. d. With 227, leſs than 1 diameter of S.; with

I. with 460, near two diameters. Position 82° 48′ f. following.

56. FL. 54^{am} Ceti ſequens ad auſtrum.

Oct. 12, 1782. Double. About $\frac{1}{3}$ degree f. following the 54th, towards δ Ceti. Nearly equal. Both r. With 227, about 1 diameter; with 460, about 1$\frac{1}{2}$ diameter. Poſition 87° 39′ n. following.

57. FL. 70^{am} et 67^{am} Orionis præiens.

Oct. 12, 1782. Multiple. In a ſpot which appears nebulous in the finder, and is about 50′ from the 67th, and 45′ from the 70th Orionis. More than 12 ſtars in view with 460; among them is a double ſtar. The largeſt of the baſe of an iſoſceles triangle, n. preceded by four ſtars in a line. Conſiderably unequal. With 460, 1 full diameter of L. Poſition 19° 48′ f. following..

58. δ Lyræ (FL. 12^{am}) ſequens. Inter eductionem cornuum.

Oct. 24, 1782. Double. About $\frac{1}{2}$ degree following the 12th, in a line continued from the 11 through the 12th Lyræ; the laſt of a ſmall teleſcopic triangle. Extremely unequal. L. r.; S. d. Not eaſily ſeen with 227; with 460, near 2 diameters of L. Poſition 13° 0′ n. preceding.

59. Ab ι (FL. 18^{a}) Lyræ β verſus.

Oct. 24, 1782. Double. The moſt ſouth of two very ſmall teleſcopic ſtars, which are the ſecond pair ſituated in a line from ι towards β Lyræ. A little unequal. Both d.; the fainteſt object that can be imagined. With 460, about ι diameter. Poſition 75° 0′ f. preceding; the meaſure is liable to ſome error from the obſcurity.

60. E teleſcopicis γ et λ Lyræ auſtralioribus et ſequentibus.

Double

I.

Oct. 24, 1782. Double. About ¾ degree ſ. following λ, in a line parallel to α and γ Lyræ; a very ſmall teleſcopic ſtar. Extremely unequal. Both dr. With 227, 1 full diameter of L; with 460, near 2 diameters of L. Poſition 16° 48′ n. preceding.

61. Præiens Fl. 1am Equulei.

Oct. 26, 1782. A minute double ſtar. About ¾ degree n. preceding the 1ſt Equulei, in a line parallel to α Equulei and γ Aquilæ; a large ſtar. Very unequal. Both pr. With 460, ½ diameter of S. Poſition 18° 24′ n. preceding. A pretty object, but requires fine weather.

62. Sequitur Fl. 2am Equulei.

Oct. 29, 1782. Double. About ¾ degree ſ. following the 2d Equulei, in a line parallel to δ Delphini and δ Equulei. Conſiderably unequal. Both r. With 460, 1¼ or 1½ diameter of S. Poſition 35° 9′ ſ. preceding.

63. γ Equulei (Fl. 5â) auſtralior.

Oct. 29, 1782. Double. Full ½ degree ſ. of γ, in a line from the 5th through the 6th Equulei continued. Equal. Both dr. With 227, about ¼ diameter ſcarce viſible; with 460, about ½ diameter. Poſition 5° 57′ ſ. preceding.

64. π Arietis. Fl. 42. In poplite.

Oct. 29, 1782. Treble. Exceſſively unequal. L. w; S. both mere points. With 227, neither of the ſmall ſtars can be ſeen, except with conſiderable and long continued attention, when they alſo appear; the neareſt with this power is ¾ or ⅘ diameter of L.; with 460, 1½ or 1¾ diameter of L. The third is about 25″ or 26″ diſtant from L, by exact eſtimation. Poſition of both, being all three in a line 19° 19′ ſ. following; as exact as the obſcurity will permit.

I. 65. In Nubecula β Sagittæ adjecta et ſequenti.

Nov. 4, 1782. Double. $\frac{1}{3}$ degree n. following β Sagittæ, towards 29th Vulpeculæ; the largeſt and moſt ſouth of a cluſter of ſmall ſtars that appear cloudy in the finder. Very unequal. L. rw.; S. pr. With 227, full 1 diameter of L.; with 460, about $1\frac{3}{4}$ or 2 diameters of L. Poſition 14° 0′ n. preceding. A third ſtar in view, of the 5th or 6th claſs.

66. β (FL. 23^{a}) Draconis auſtralior et præcedens.

Nov. 4, 1782. Double. About $1\frac{1}{4}$ degree ſ. preceding β, in a line from ν continued through β Draconis. Pretty unequal. Both pr. With 460, $1\frac{1}{2}$ or $1\frac{3}{4}$ diameter of L. Poſition 2° 24′ ſ. preceding.

67. Nebulam Aurigæ pedem dextrum ſequentem, præcedens.

Nov. 4, 1782. Double. About 55′ from the 37th Nebula of M. MESSIER; the largeſt and moſt preceding of two ſtars. Very unequal. Both pr. With 460, near 2 diameters of L. Poſition 23° 57′ n. following.

68. Parvula FL. $10^{æ}$ Orionis quam proximè adjecta.

Nov. 5, 1782. Double. The ſmall ſtar not many minutes from the 10th Orionis. A little unequal. Both whitiſh. With 460, near 1 diameter. Poſition 84° 54′ ſ. following; a little inaccurate on account of the difficulty of ſeeing the ſtars well.

69. In Lyncis pectore.

Nov. 13, 1782. Double. About 3 degrees ſ. preceding the 19th Lyncis, in a line drawn from the 19th Lyncis to τ Aurigæ; the 24th and 19th Lyncis alſo point to it nearly: in a very clear evening it may juſt be ſeen with the naked eye. A little unequal. Both rw. With 227, $\frac{3}{4}$ dia-

I. $\frac{3}{4}$ diameter; with 460, $1\frac{1}{2}$ or near $1\frac{1}{2}$ diameter. Position 77° 0′ f. following.

70. ζ (Fl. 123ª) Tauri borealior et præcedens.

Nov. 13, 1782. A very pretty double ſtar. Near 1 degree n. preceding ζ Tauri towards Capella; the corner of a rhomboid made up of ζ, this, and two more, and oppoſite to ζ. Conſiderably unequal. L. pr.; S. a little deeper r. With 227, almoſt 1 diameter of L.; with 460, $1\frac{3}{4}$ diameter of L. Poſition 36° 24′ f. preceding.

71. Fl. 44ªm Urſæ majoris præcedens ad auſtrum.

Nov. 19, 1782. Double. Nearly in the interſection of a line from β Urſæ majoris to the 39th Lyncis, croſſed by one from ψ to υ Urſæ majoris; the laſt line ſhould bend a little towards ψ Urſæ majoris. A little unequal. Both whitiſh. With 460, near 2 diameters of S. Poſition 2° 6′ n. following.

72. Fl. 65. Urſæ majoris.

Nov. 20, 1782. Double. Exceſſively unequal. L. pr.; S. a point. Not viſible with 227, nor hardly to be ſuſpected unleſs it has been firſt ſeen with a higher power; with 460, $1\frac{3}{4}$ diameter of L. or, when long viewed, full 2 diameters of L. Poſition 53° 45′ n. following. A third ſtar in view. Equal to L. Colour rw. Diſtance 1′ 0″ 4‴. Poſition 22° 21′ f. following.

73. β (Fl. 6ª) Arietis borealior et præcedens.

Nov. 22, 1782. Double. About $1\frac{3}{4}$ degree n. preceding β Arietis, towards β Andromedæ; a conſiderable ſtar. Very unequal. L. r.; S. deeper r. With 227, about $\frac{3}{4}$ diameter of L.; with 460, full $1\frac{1}{4}$ or almoſt $1\frac{1}{2}$ diameter of L. when beſt. Poſition 77° 24′ f. following.

I. 74. FL. 39^{a} Arietis borealior et præcedens.

Dec. 22, 1782. Double. About $\frac{2}{3}$ degree n. preceding 39 Arietis, towards γ Trianguli; a pretty large teleſcopic ſtar. A little unequal. Both pr. With 227, near 1 diameter of L.; with 460, about $1\frac{1}{2}$ diameter of L. Poſition 20° 36′ n. preceding.

75. FL. 26^{am} Orionis præcedens ad auſtrum.

Jan. 9, 1783. Double. About $\frac{1}{4}$ degree ſ. preceding the 26th, in a line parallel to δ and β Orionis; the fartheſt of two; or $\frac{3}{4}$ degree ſ. preceding the 30th in the ſame direction. Nearly equal. Both w. or rw. With 460, perhaps a diameter. Poſition 89° 36′ n. preceding; but not very accurate.

76. In pectore Lyncis.

Jan. 23, 1783. Double. Not eaſy to be found. A line from the 19th Lyncis to υ Geminorum croſſed by one from θ Urſæ majoris to ε Aurigæ, points out a ſtar but juſt viſible in a fine evening; it is perhaps about three degrees from the 19th Lyncis; when that ſtar is found, we have the double ſtar about 1 degree n. following the ſame, in a line parallel to τ Geminorum and the 19th Lyncis. Conſiderably unequal. Both aſh w. With 460, $\frac{1}{4}$ diameter of S. Poſition 0° 0′ preceding. A third large ſtar in view. Diſtance 1′ 7″ 46‴. Poſition 3° 42′ ſ. preceding.

77. α (FL. 7^{a}) Crateris borealior.

Jan. 31, 1783. Double. Near $2\frac{3}{4}$ degrees north of α Crateris; a ſmall teleſcopic ſtar, about $\frac{1}{4}$ degree following the moſt north of two large ones. Pretty unequal. Both whitiſh. With 227, leſs than half diameter of S.; with

I. with 460, near 1 diameter; with 625, a little more than 1 diameter. Poſition 82° 24′ n. following.

78. Fl. 11ª Libræ borealior.

Jan. 31, 1783. Double. Near 2½ degrees north of the 11th Libræ, in a line parallel to μ Virginis and the 109th of the ſame conſtellation. Equal. Both inclining to r. With 460, full 1 diameter. Poſition 58° 24′ n. preceding, or ſ. following.

79. Fl. 46 Herculis. In dextro latere.

Feb. 5, 1783. Double. Extremely or almoſt exceſſively unequal. L. w.; S. d. With 227, it is hardly viſible; with 460, near 1 diameter of L. Poſition 66° 36′ ſ. following.

80. Fl. 81 Virginis.

Feb. 7, 1783. Double. Equal. Both pr. With 227, near ½ diameter; with 460, ⅔ diameter. Poſition 41° 12′ n. following or ſ. preceding.

81. π Serpentis (Fl. 44ᵃᵐ) præcedens ad auſtrum.

Mar. 7, 1783. Double. About 1¼ degree ſ. preceding π, towards ϰ; the moſt north of two. A little unequal. Both r. With 460, 1½ diameter of L. Poſition 49° 48′ ſ. preceding. A third large ſtar in view; paler than the other two. Diſtance from the two taken as one ſtar 56″ 28‴. Poſition, with L. of the two, 31° 48′ ſ. preceding.

82. Fl. 49 Serpentis.

Mar. 7, 1783. Double. The moſt north and following of two ſtars. A little unequal. Both pr. With 227, ¼ or ⅓ diameter, and a very minute and beautiful object; with 460, ¾ diameter. Poſition 21° 33′ n. preceding.

L. 83. λ Ophiuchi. FL. 10. In ancone ſiniſtri brachii.

Mar. 9, 1783. A very beautiful and cloſe double ſtar. L. w.; S. blue; both fine colours. Conſiderably or almoſt very unequal. With 460, ¼ or ⅓ diameter of S.; with 932, full ⅓ diameter of S. Poſition 14° 30′ n. following.

84. FL. 50ª Aurigæ auſtralior.

Mar. 18, 1783. Double. Near 1 degree ſ. of the 50th Aurigæ, in a line parallel to β and θ. Very unequal. L. r.; S. dr. With 227, about ¾ diameter of L.; with 460, almoſt 1¼ diameter of L. Poſition 14° 0′ n. following.

85. FL. 36ªm Lyncis ſequens ad auſtrum.

Mar. 24, 1783. Double. Near ½ degree ſ. following the 36th Lyncis, in a line parallel to the 31ſt Lyncis and *n* Urſæ majoris; of two the neareſt to the 31ſt Lyncis. Conſiderably unequal. Both w. With 227, 1 diameter of L.; or when long kept in view, 1¼ diameter of L.; with 460, and after long looking, 2 diameters of L; otherwiſe not near ſo much. Poſition 88° 57′ n. following.

86. FL. 105ª Herculis borealior.

Mar. 27, 1783. Double. One full degree n. of the 105th Herculis, in a line from the 72d Serpentarii continued through the 105th Herculis; a ſmall teleſcopic ſtar. Conſiderably unequal. Both dr. With 460, a little more than 1 diameter of L. Poſition 79° 24′ n. preceding.

87. *q* Ophiuchi. FL. 73.

April 27, 1783. A very minute double ſtar. Conſiderably unequal. L. r. S. r. With 227, not to be ſuſpected unleſs known to be double, but may be ſeen wedge-formed, and with

I. long attention I have alſo perceived a moſt minute diviſion; with 460, about $\frac{1}{4}$ or $\frac{1}{3}$ diameter of S. Poſition 2° 48′ ſ. preceding.

88. τ Ophiuchi. Fl. 69. In dextra manu ſequens.

April 28, 1783. The cloſeſt of all my double ſtars; can only be ſuſpected with 460; but 932 confirms it to be a double ſtar. Pretty unequal. Both pr. or wr. It is wedge-formed with 460; with 932, one-half of the ſmall ſtar, if not three-quarters ſeem to be behind the large ſtar. Poſition of the wedge 61° 36′ n. preceding. ν Ophiuchi, juſt by, is perfectly free from this wedge-formed appearance.

89. Illas ad Fl. 56^{am} Andromedæ præcedens ad boream.

July 28, 1783. Double. About $\frac{2}{3}$ degree preceding, and a little north of the two ſtars that are about the place of the 56th Andromedæ, in a line towards μ; a conſiderable ſtar; and of two in a line parallel to β and γ Trianguli that which is neareſt to the 56th Andromedæ. Pretty unequal. L. drw.; S. dpr. With 227, near 1 diameter of L.; with 460, about $1\frac{1}{2}$ diameter of L. Poſition 75° 30′ ſ. following.

90. β Aquarii (Fl. 22^{am}) præcedens ad auſtrum.

July 31, 1783. Double. About $4\frac{1}{2}$ degrees from β towards μ Aquarii. A little unequal. Both dw. or pr. With 460, $1\frac{1}{2}$ diameter or near 2. Poſition 77° 36′ ſ. following.

91. γ Aquilæ (Fl. 50^{am}) præcedens ad boream.

Aug. 7, 1783. Double. About $\frac{1}{3}$ degree n. preceding γ, in a line parallel to γ and ζ Aquilæ; of two that neareſt to γ. Very unequal. L. dpr.; S. d. With 227, hardly viſible, and like a ſtar not in focus; with 460, appears nebulous.

I. nebulous on one ſide, but is a double ſtar; with 932, about 1½ diameter of L. Poſition 8° 18′ n. preceding.

92. π Aquilæ. FL. 52. Duarum in ſiniſtro humero ſequens.

Aug. 27, 1783. A minute pretty double ſtar. A little unequal. Both pr. With 460, ½ diameter of L. or near ¾ diameter of S. Poſition 34° 24′ ſ. following.

93. FL. 62am Aquilæ præcedens ad boream.

Sept. 12, 1783. A minute double ſtar. About ¾ degree n. preceding the 62d, in a line parallel to θ and ζ Aquilæ; a pretty conſiderable ſtar. Very unequal. Both inclining to pr. With 278, almoſt in contact; with 460, near ¾ diameter of S.; when in the meridian, and the air fine, near 1 diameter of L. Poſition 19° 9′ n. preceding.

94. δ Cygni. FL. 18. In ancone alæ dextræ.

Sept. 20, 1783. Double. Very unequal. L. fine w.; S. aſh colour inclining to r. With 278, about ½ diameter of L.; with 460, ¾ diameter of L.; with 932, full 1½ diameter of L. in hazy weather, which has taken off the rays of L. and and thereby increaſed the interval. Poſition 18° 21′ n. following; perhaps a little inaccurate.

95. FL. 33am Cygni ſequens ad auſtrum.

Sept. 22, 1783. Double. Full 1⅓ degree ſ. following the 33d, towards ξ Cygni; a pretty conſiderable ſtar. Very unequal. L. w.; S. inclining to r. With 460, at firſt about ⅔ diameter of L.; but, after looking a conſiderable time, and in a fine air, near 1½ diameter. Poſition 72° 15′ n. preceding.

96. η (FL. 21am) Cygni ſequens ad auſtrum.

Sept. 23, 1783. Treble. Full 1¾ degree n. following η, in a line parallel to β and λ Cygni. The two neareſt conſiderably unequal.

I. unequal. Both pr. With 460, 1 diameter of S. or ¾ diameter of L. Poſition 89° 18′ ſ. following. The two fartheſt conſiderably unequal; the colour r. Diſt. . . . . Poſition 56° 3′ n. preceding.

97. FL. 51am Cygni ſequens.

Sept. 24, 1783. A minute double ſtar. About 2½ degrees following the 51ſt, in a line parallel to δ and α Cygni; the largeſt and moſt ſouth of an obtuſe-angled triangle; a very conſiderable ſtar. Pretty unequal. Both rw.; but S. a little darker r. With 278, ½ diameter of S. and beautiful; with 460, ¾ diameter of S. Poſition 46° 24′ n. following.

SECOND CLASS OF DOUBLE STARS.

H. 39. Procyonem juxta.

Feb. 2, 1782. Double. About 2 degrees ſ. following Procyon, in a line from λ Geminorum continued through Procyon. Exceſſively unequal. L. pr.; S. not viſible with 278; with 460, more than 3 diameters of L. Poſition, by the aſſiſtance of a wall * and micrometer 54° 28′ ſ. following.

40.

* When the ſmall ſtar is ſo faint as not to bear the leaſt illumination of the wires, its poſition may ſtill be meaſured by the aſſiſtance of ſome wall or other object; for an eye which has been ſome time in the dark, can ſee a wall in a ſtar-light evening ſufficiently well to note the projection of the ſtars upon it, in the manner

II. 40. * Secunda ad φ Cancri. FL. 23.

Feb. 2, 1782. Double. A little unequal. Both rw. With 227, near 2 diameters; with 460, 2½ diameters of L. Position 56° 42′ n. following.

41. * Prima ad υ Cancri. FL. 24.

Feb. 2, 1782. Double. Conſiderably unequal. Both pr. With 227, 1½ diameter of L.; with 460, 4 diameters of L. Poſition 32° 9′ n. following.

42. E teleſcopicis *k* Virginis precedentibus †.

Feb. 6, 1782. Double. About 1¼ degree ſ. preceding *k* Virginis, in a line parallel to ζ and θ; the moſt ſouth of three forming an arch. Extremely unequal. L. w.; S. hardly viſible with 227 (but with a ten-feet reflector S. b.); with 460, above 2 diameters of L. Poſition 52° 24′ ſ. following.

43. FL. 43^{am} Leonis præcedens ad auſtrum. In dextro genu.

Feb. 17, 1782. Double. Near ⅔ degree ſ. preceding the 43d, in a line parallel to α and the 14th Leonis. Very unequal. L. w.; S. d. With 227, near 2¼ diameters of L. when beſt. Poſition 85° 2′ n. following.

44. 9 Virginis. FL. 84. Verſus finem alæ dextræ.

Feb. 17, 1782. Double. Extremely unequal. L. w. inclining to r.; S. d. Requires attention to be ſeen with 227; with 460, 2½ diameters of L. Poſition, with 278, 29° 5′ ſ. preceding.

which has been deſcribed with the lamp-micrometer, Phil. Tranſ. vol. LXXII. p. 169 and 170. Then, introducing ſome light, and adapting the fixed wire to the obſerved direction of the ſtars on the wall, the moveable wire may be ſet to the parallel of the large ſtar, which will give the angle of poſition pretty accurately.

† See note to IV. 51.

II. 45. Fl. 54 Virginis.

April 3, 1782. Double. A little unequal. Both w. With 227, $1\frac{1}{2}$ or near $1\frac{3}{4}$ diameter. Position 57° 0′ n. following.

46. Fl. 42^{am} Comæ Berenices ſequens ad auſtrum.

April 15, 1782. Double. About $1\frac{1}{4}$ degree from the 42d Comæ towards υ Bootis; the moſt ſouth of a teleſcopic equilateral triangle. Exceſſively unequal. L. pr.; S. d. With 278, $2\frac{1}{2}$ diameters of L.; not ſo well to be ſeen with higher powers. Poſition 6° 42′ ſ. following. A third ſtar preceding, above 1′.

47. Fl. 2 Comæ Berenices.

April 18, 1782. Double. Conſiderably unequal. L. rw.; S. pr. With 278, 2 diameters of L; with 460, above 2 diameters of L. Poſition 27° 42′ ſ. preceding.

48. Prope Fl. 16^{am} Aurigæ.

Aug. 28, 1782. A minute double ſtar. Leſs than $\frac{1}{4}$ degree ſ. preceding the 16th, in a line parallel to the 10 and 8 Aurigæ; the preceding ſtar of a ſmall triangle of which the 16th is the largeſt and following. A little unequal. Both pr. With 227, $1\frac{1}{2}$ or, when beſt, $1\frac{3}{4}$ diameter of L. Poſition 15° 48′ n. following.

49. *o* (Fl. 110^{a}) Piſcium borealior. In lino boreo.

Sept. 3, 1782. Double. About $\frac{1}{2}$ degree n. of, and a little preceding 110th, towards η Piſcium. A little unequal. Both wr. With 460, about 3 diameters of L. Poſition 59° 6′ n. preceding. A third ſtar in view, about $1\frac{3}{4}$ min.

50. Fl. 38. Piſcium. In auſtrino lino.

Sept. 4, 1782. Double. Pretty unequal. Both pr. With 227, full 2 diameters of L.; with 460, about 4 diameters of L. Poſition 25° 3′ ſ. preceding.

II. 51. ρ Capricorni. FL. 11. Trium in roſtro ſequens.

Sept. 5, 1782. Double. Very unequal. Both rw. With 460, 1½ diameter of L. Poſition 84° 0′ ſ. following. A third ſtar in view.

52. *o* (FL. 40^{am}) Perſei præcedens ad boream.

Sept. 7, 1782. Double. Almoſt ½ degree preceding the 40th, in a line parallel to ζ and the 38th Perſei. Equal. Both w. With 227, nearly 2 diameters. Poſition 8° 24′ n. preceding.

53. FL. 12^{am} Camelopardali præcedens.

Sept. 7, 1782. Double. Leſs than ¼ degree preceding the 11th and 12th, in a line from the 1ſt Lyncis continued through the 12th Camelopardali. Extremely unequal. Both dr. With 227, it appears like a ſtar with a tail; but 932 ſhews it plainly to be only a double ſtar; with 227, not much above 1 diameter of L.; with 932, about 3½ diameter of L. Poſition 18° 33′ ſ. following; a little inaccurate.

54. Quæ præcedit ε (FL 74^{am}, oculum boreum) Tauri.

Sept. 7, 1782. Double. Near ½ degree ſ. preceding ε, in a line parallel to α and γ Tauri; a ſmall ſtar. Extremely unequal. L. rw.; S. d. With 460, above 3 diameters of L. Poſition 68° 42′ ſ. preceding.

55. FL. 4^{a} Ceti auſtralior et ſequens.

Sept. 9, 1782. Double. About 1 degree ſ. following the 4th and 5th in a line parallel to η and τ Ceti; in the ſhorter leg of a rectangular triangle. Very unequal. L. r.; S. d. With 278, rather more than 2 diameters. Poſition 21° 42′ n. preceding.

56. β (FL. 6^{am}) Arietis præcedens ad boream.

II. Double. Almoſt 1 degree n. preceding β Arietis,
Sept. 10, towards ζ Andromedæ; a ſmall ſtar. A little unequal.
1782. Both reddiſh. With 227, full 2 diameters of L. Poſition 23° 12′ n. preceding. A third ſtar 2′ or 3′ preceding, in the ſame direction with the two ſtars of the double ſtar.

57. Ad Fl. 72^{am} Aquarii.
Sept. 27, Treble. About 2½ degrees following ϰ, in a line parallel
1782. to α and η Aquarii. The neareſt a little unequal. Both r. With 460, 2½ diameters of L. Poſition 25° 51′ f. preceding. The two fartheſt a little unequal. Of the 5th claſs. About 50° or 55° f. following.

58. Fl. 56^{a} Ceti auſtralior et ſequens.
Sept. 27, Double. About ¾ degree f. following the 56th, in a
1782. line parallel to η and τ Ceti. Conſiderably unequal. Both dw. With 278, 1½ diameter of L. Poſition 25° 12′ n. preceding; too low for accuracy.

59. ρ (Fl. 46^{am}) Aquarii ſequens ad auſtrum.
Sept. 30, Double. About 2 degrees f. following ρ, in a line pa-
1782. rallel to β and δ Aquarii; there is a very conſiderable ſtar between this and ρ, not much out of the line. Pretty unequal. Both dr. With 227, 2½ or 2¾ diameter of L. Poſition 61° 12′ n. preceding.

60. ξ (Fl. 5^{am}) Canis majoris ſequens ad boream.
Sept. 30, Double. About ½ degree n. following the 2d ad ξ,
1782. in a line from the 4th continued through the 5th Canis majoris nearly. Very unequal. L. rw.; f. d. With 227, 1½ diameter. Poſition 67° 36′ n. preceding.

61. ϖ (Fl. 47^{am}) Orionis ſequens ad auſtrum.
Oct. 2, Treble. About 1½ degree f. following ϖ in a line
1782. parallel to φ and α Orionis; the ſmalleſt and moſt ſouth of three forming an arch. The two neareſt extremely unequal.

H. unequal. L. dw.; S. a mere point. With 227, 1½ or 1¾ diameter of L. Position 4° 54′ n. following; too obscure for accuracy. The two farthest extremely unequal. S. a mere point. Of the fourth class. Position about 50° f. following.

62. FL. 3ᵉ Pegasi adjecta.

Oct. 4, 1782. Double. In a line with, and north of, the two stars that are about the place of the third Pegasi. A little unequal. Both dusky r. With 227, about 3 diameters of S. Position 88° 24′ n. preceding; perhaps a little inaccurate.

63. FL. 2ᵃᵐ et 4ᵃᵐ Navis præcedens.

Oct. 12, 1782. Multiple. Near 2 degrees preceding the 2d and 4th Navis; the middle one of three. One of the multiple is double. Nearly equal. Both w. or ash colour. With 227, about 2½ diameter, and not less than 20 stars more in view; with 460, about 3 diameters. Position 30° 12′ n. preceding.

64. *g* (FL. 81ᵃᵐ) Geminorum ad austrum sequitur.

Oct. 13, 1782. Double. About ½ degree f. following *g*, in a line from ζ continued through *g* Geminorum nearly; the nearest and largest of two. Very unequal. L. r.; S. bluish r. With 227, above 3 diameters of L. Position 4° 9′ n. preceding.

65. Pollucem sequens ad boream.

Oct. 13, 1782. Double. Full ¾ degree n. following β, in a line from δ continued through β Geminorum; the star next to the middle one of three, nearly in a line. Excessively unequal. L. rw.; S. d. With 227, above 2½ or near 3 diameters of L. and 5 other stars in view; with 460, above 3 diameters of L. Position 89° 12′ n. following.

66.

H. 66. Juxta γ Delphini.

Oct. 19, 1782. Double. Full $\frac{1}{4}$ degree f. preceding γ, towards δ Delphini. Conſiderably unequal. L. pr.; S. r. With 227, $1\frac{1}{2}$ diameter of L. Poſition 78° 42′ n. preceding.

67. β (Fl. 10^{am}) Lyræ præcedens ad boream.

Oct. 19, 1782. Double. The 4th teleſcopic ſtar about $1\frac{1}{2}$ degree n. preceding β, in a line parallel to γ and α Lyræ. Extremely unequal. L. r.; S. dr. With 227, $1\frac{1}{4}$ or almoſt $1\frac{1}{2}$ diameter of L. With 460, above 2 diameters of L. Poſition 68° 6′ f. following.

68. Proximè ϱ Lyræ.

Oct. 24, 1782. Treble. About $2\frac{1}{4}$ minutes f. following ϱ Lyræ. The two neareſt, a little unequal. Both dr. With 460, 3 full diameters. Poſition 8° 24′ n. following. The fartheſt as large as L. of the two neareſt at leaſt. Colour dr. Poſition with L. 25° 57′ f. preceding. Diſtance of ϱ Lyræ, which is in view, from the two neareſt 2′ 17″ 30‴. Poſition 65° 12′, ϱ being n. preceding, or the double ſtar f. following.

69. Fl. 4^{am} Cygni ſequens ad boream.

Oct. 24, 1782. Double. Near $\frac{1}{2}$ degree n. following the 4th Cygni, in a line from γ Lyræ continued through the 4th Cygni. A little unequal. Both w. With 227, about 2 diameters of L. or $2\frac{1}{2}$ when beſt. Poſition 29° 12′ n. following.

70. τῶν 8 teleſcopicarum $\varkappa$ (Fl. 15.) Sagittæ ſequentium ultima.

Nov. 6, 1782. Double. About $1\frac{1}{4}$ degree f. following $\varkappa$ Sagittæ, in a line parallel to γ Sagittæ and γ Delphini. Extremely unequal. Both r.; S. deeper r. With 227, $1\frac{1}{2}$ diameter

II. meter of L.; with 460, above 2 diameters of L. Poſition 72° 57′ n. following.

71. FL. 58ª Aurigæ auſtralior.

Nov. 6, 1782. Multiple. About ¾ degree ſ. of the 58th Aurigæ, in a line parallel to β and θ. A cluſter of ſtars containing a double ſtar of the ſecond, and one of the third claſs. That of the ſecond very unequal. Both r. With 460, about 2½ diameter of L. Poſition 44° 36′ n. following; that of the third equal. Both r. With 227, above 20 ſtars in view. Diſtance 17″ 41‴. The two double ſtars are in the following ſide of a ſmall teleſcopic trapezium.

72. FL. 13ª Lyncis auſtralior.

Nov. 13, 1782. A pretty double ſtar. About 1¼ degree ſ. of the 13th Lyncis, towards θ Geminorum; a conſiderable ſtar. Nearly equal. Both pr. With 227, full 2½ diameters; with 460, almoſt 4 diameters. Poſition 11° 0′ ſ. preceding.

73. FL. 21ª Urſæ majoris.

Nov. 17, 1782. Double. Very unequal. Both rw. With 227, 2¼ diameter of L.; with 460, above 3. Poſition 36° 45′ n. preceding.

74. ν (FL. 4ª) Crateris borealior.

Nov. 20, 1782. Treble. Near 1 degree n. preceding ν Crateris, towards α Leonis. The two neareſt equal. Both dw. With 227, 2½ or 3 diameters. Poſition 71° 33′ n. following. The fartheſt larger than either of the two other ſtars. Of the ſixth claſs. Poſition about 68 or 69° ſ. preceding the double ſtar.

II. 75. Fl. 118 Tauri.

Dec. 7, 1782. Double. A little unequal. L. w.; S. w. inclining to r. With 278, $2\frac{1}{2}$ diameter of L.; with the ſame power by the micrometer $4''\ 41'''$; more exactly with 625, $5''\ 2'''$. Poſition 77° 15′. I could juſt ſee it with an 18-inch achromatic, made by Mr. Nairne; it was as cloſe as poſſible, and a pretty object.

76. τ (Fl. 63^{a}) Arietis auſtralior et præcedens.

Dec. 23. 1782. Double. About 1 degree ſ. preceding τ Arietis, towards μ Ceti; the moſt ſouth of two ſmall teleſcopic ſtars. Nearly equal. Both w. With 227, above 3 diameters; by the micrometer $5''\ 47'''$. Poſition 15° 24′ ſ. preceding.

77. * Fl. 17 Hydræ.

Dec. 28, 1782. Double. The largeſt of two. A little unequal. Both w. With 227, $2\frac{1}{4}$ diameter of L.; with 460, $1\frac{3}{4}$ diameter. Poſition 90° 0′ north.

78. χ (Fl. 63^{am}) Leonis ſequens ad auſtrum.

Jan. 1, 1783. Double. About $\frac{1}{3}$ degree ſ. following χ, towards τ Leonis; the ſmalleſt of two. Very or extremely unequal. L. r.; S. d. With 227, 3 full diameters of L. Poſition 75° 21′ ſ. following.

79. Fl. 39 Bootis.

Jan. 8, 1783. A pretty double ſtar. A little unequal. Both pr. With 227, near $1\frac{1}{2}$ diameter of L.; with 460, near 2 diameters of L. Poſition 38° 21′ n. following.

80. *d* (Fl. 40^{x}) Eridani adjecta.

Jan. 31, 1783. Double. About $1\frac{1}{3}$ min. ſ. following *d* Eridani. Very unequal. Both dr. With 227, hardly viſible; with 460, very obſcure. Poſition 56° 42′ n. preceding.

II. Distance of L. from *d* Eridani, with 227, 1′ 21″ 47‴.
Position of L. 17° 53′ f. following *d* Eridani.

81. FL. 49^{am} Eridani sequens.

Jan. 31, 1783. Double. Near 1 degree following the 49th Eridani, towards δ Orionis. Very unequal. Both dw. With 227, full 1 diameter of L.; with 278, 1½ or 1¾ diameter of L.; with 460, 2½ or 3 diameters of L. Position 51° 36′ n. preceding.

82. FL. 31^{am} Bootis sequens ad austrum.

Feb. 3, 1783. Double. Near 1 degree f. following the 31st, in a line from *v* continued through the 31st Bootis; the most south of two. A little unequal. L. w.; S. dw. With 227, about 1¾ diameter of L.; with 460, about 3 diameters of L. Position 1° 0′ f. following. A third star in view, 20° or 30° n. preceding.

83. FL. 22^{â} Andromedæ borealior.

Feb. 26, 1783. Double. Within ½ degree north of the 22d, in a line parallel to the 19th and 16th Andromedæ; the following and smallest of two. Considerably unequal. L.w.; S. d. With 227, 1¼ or 1½ diameter of L.; with 460, more than 2 diameters of L. Position 5° 48′ n. following.

84. FL. 65 Piscium.

Feb. 27, 1783. Double. Nearly equal. Both pr. With 227, near 1½ diameter of L.; with 460, full 2 diameters. Position 30° 57′ n. preceding.

85. *b* (FL. 36^{â}) Serpentis borealior et sequens.

Mar. 4, 1783. Double. About 1½ degree n. following *b*, nearly in a line from the 32d continued through the 36th Serpentis. Extremely unequal. L. w.; S. dw. With 227, 1 full diameter of L.; S. hardly to be seen; with 460, full 2 diameters of L. Position 46° 9′ n. preceding.

86.

II. 86. Fl. 49^{am} Serpentis præcedens ad auſtrum.

Mar. 7, 1783. Double. About 1½ degree ſ. preceding the 49th, in a line with the 49th and another between this and the 49th Serpentis, each nearly at ¾ degree diſtance. Very unequal. L. dw.; S. d. With 227, 2 diameters, or 2¼ when beſt. Poſition 53° 9′ ſ. following.

87. Fl. 29^{a} et 30^{a} Monocerotis auſtralior.

Mar. 8, 1783. Multiple. It makes nearly an equilateral triangle with the 29th and 30th Monocerotis towards the ſouth. Among many, the fourth from the ſouth end of an irregular long row is double. A little unequal. Both pr. With 227, 1 diameter of L. and 16 more in view. Poſition 86° 12′ ſ. following.

88. ω (Fl. 51^{am}) Serpentis præcedens ad auſtrum.

Mar. 8, 1783. Double. About ½ degree ſ. preceding the 51ſt, towards the 13th Serpentis. Very or extremely unequal. Both r. With 227, 2¼ diameter of L. when beſt; with 460, near 3 diameters of L. Poſition 44° 45′ n. preceding.

89. Ad Genam Monocerotis.

Mar. 26, 1783. Double. About 1 degree n. preceding the 12th Monocerotis, in a line parallel to α and λ Orionis; the ſmalleſt and moſt north of two. Conſiderably unequal. L. r.; S. bluiſh r. With 227, near 4 diameters of L. when beſt. Poſition 50° 51′ n. following.

90. Fl. 100^{am} Herculis præcedens ad boream.

Mar. 27, 1783. Double. About 1¾ degree n. preceding the 100th, towards μ Herculis; a very ſmall teleſcopic ſtar; the moſt towards μ and ſmalleſt of three forming an arch. Conſiderably unequal. Both dw. With 227, about 2 diameters of L. Poſition 75° 9′ ſ. following.

II. 91. ζ (FL. 15ª) Sagittæ auſtralior.

Apr. 5, 1783. Treble. About twice as far ſouth of ζ Sagittæ, as ζ and the ſtar near it are from each other; a ſmall ſtar. The two neareſt very unequal. L. pr.; S. r. With 227, 1½ diameter of L. Poſition 74° 54′ f. preceding. The third with L. extremely unequal. S. d. With 227, about 3 diameters of L. or more. Poſition about 40° or 50° n. preceding. With more light this would be a fine object.

92. In Camelopardali clune.

Apr. 30, 1783. Double. About four times the diſtance of the 10th and 12th Camelopardali, north of the 10th, and almoſt in the ſame direction with the 10th and 12th, is a ſtar of between the 5th and 6th magnitude not marked in FLAMSTEED; naming that ſtar A, we have the following direction. About ½ degree preceding A Camelopardali, in a line from the 2d Lyncis continued through A; the ſecond from A. Very unequal. L. w.; S. d. With 227, 1½ or 2 diameters of L. Poſition 22° 42′ f. following. Very inaccurate.

93. ε (FL. 13ª) Aquilæ auſtralior.

May 25, 1783. Double. Near ¼ degree ſouth of, and a little following ε, towards λ Aquilæ, a very ſmall ſtar. Very unequal. L. dw.; S. dr. With 460, above 2 diameters of L. Poſition 16° 0′ n. preceding.

94. ι (FL. 17ªᵐ) Andromedæ præcedens ad boream.

Aug. 19, 1783. Double. About 1⅓ degree n. preceding ι Andromedæ in a line parallel to α and β Caſſiopeiæ; in the ſide of a trapezium of four ſmall ſtars. Pretty unequal. Both r. With 460, 2½ diameters of L. Poſition 34° 24′ n. preceding.

95.

II. 95. η (Fl. 55^{h}) Aquilæ auſtralior.

Sept. 12, 1783. Double. About $\frac{1}{3}$ degree ſouth of η, in a line from α continued through η Aquilæ; a ſmall ſtar. A little unequal. Both duſky aſh-coloured. With 460, near 3 diameters of L.; with 278, near 2 diameters of L. Poſition 29° 3′ n. preceding.

96. θ (Fl. 65^{h}) Aquilæ borealior et ſequens.

Sept. 12, 1783. Double. About $1\frac{3}{4}$ degree n. following θ Aquilæ, towards ϵ Delphini; more accurate towards 29 Vulpeculæ; a very conſiderable ſtar. Nearly equal. Both rw. With 278, about $1\frac{1}{4}$ diameter of L.; with 460, full 2 diameters. Poſition 56° 12′ ſ. preceding.

97. ζ (Fl. 64^{am}) Cygni præcedens.

Sept. 15, 1783. Treble. About 1 degree preceding ζ, towards the 41ſt Cygni; a large ſtar. The two neareſt extremely unequal. L. w.; S. pr. With 460, $2\frac{1}{2}$ diameters of L. Poſition 45° 15′ n. preceding. The third with L. extremely unequal. Of the 5th or 6th claſs; about 50° ſ. preceding.

98. Fl. 49 Cygni.

Sept. 15, 1783. Double. Very unequal. L. r.; S. bluiſh r. With 278, $1\frac{1}{2}$ diameter of L.; with 460, $2\frac{1}{2}$ diameters of L. Poſition 31° 48′ n. following.

99. β (Fl. 6^{am}) Cygni ſequens ad boream.

Sept. 15, 1783. Double. Near $\frac{1}{2}$ degree n. following β, towards ξ Cygni. Very unequal. Both dw. With 278, $1\frac{1}{4}$ diameter of L.; with 460, about 2 diameters of L. Poſition 87° 48′ n. following.

100. Fl. 51^{a} Cygni borealior et ſequens.

Sept. 24, 1783. Double. Near two degrees n. following the 51ſt Cygni, in a line parallel to o Cygni and α Cephei; a

II. pretty conſiderable ſtar. Very unequal. L. w.; S. inclining to blue. With 278, extremely unequal. and 1½ diameters of L. when beſt; requires attention to be ſeen well with this power; with 460, full 2 diameters of L. or 2¼ when beſt, otherwiſe much leſs. Poſition 15° 51′ n. following.

101. FL. 57^{am} :: Camelopardali præcedens ad boream.

Sept. 26, 1783. Double. About 2 degrees n. preceding the 57 ::, towards the 42d Camelopardali; a conſiderable ſtar near three ſmaller, forming an arch. About 1 degree from the double ſtar V. 135. Conſiderably unequal. Both pr. With 278, 1⅔ diameter of L.; with 460, 2½ diameters of L. Poſition 67° 15′ n. preceding.

102. *e* (FL. 29^{a}) Orionis auſtralior et præcedens.

Sept. 27, 1783. Double. About ½ degree ſ. preceding *e*, in a line parallel to ϱ and β Orionis; the largeſt of ſeveral. Very unequal. L. pr.; S. inclining to garnet. With 278, near 2 diameters of L. With 460, 2½ diameters of L. Poſition 52° 24′ ſ. following.

THIRD CLASS OF DOUBLE STARS.

III. 47. *e* Pollucis: FL. 38 Geminorum. In calce.

Dec. 27, 1781. Double. Extremely unequal. L. rw.; S. r. Diſtance, with 460, 7″ 48‴. Poſition 89° 54′ ſ. following. Two more in view, the neareſt of them perhaps 40″; they form a rectangle nearly.

III. 48. *r* (Fl. 61am) Geminorum præcedens ad boream.

Dec. 27, 1781. Double. About ½ degree n. preceding *r*, in a line parallel to *κ* and the 60th Geminorum; near two degrees from *δ*. A little unequal. Both pr. Diſtance 6″ 15‴. Poſition 43° 54′ n. following.

49. *δ* (Fl. 4am) Hydræ præcedens ad boream.

Jan. 20, 1782. Double. About 1¼ degree n. preceding *δ*, in a line from *η* continued through *δ* Hydræ. Pretty unequal. L. r.; S. garnet. Diſtance 12″ 30‴. Poſition 62° 48′ n. following.

50. *θ* Virginis. Fl. 51. De quatuor ultima et ſequens.

Feb. 6, 1782. Treble. The two neareſt extremely unequal. L. w.; S. d. Diſtance 7″ 8‴; but inaccurate on account of the obſcurity of S. Poſition 69° 18′ n. preceding. For meaſures of the two fartheſt ſee VI. 43.

51. Fl. 88 Leonis. In dextro clune.

Feb. 9, 1782. Double. Extremely unequal. L. rw.; S. r. Diſtance 14″ 38‴; a little inaccurate. Poſition 47° 33′ n. preceding.

52. Fl. 10am Orionis ſequens.

Feb. 17, 1782. Double. Above ¾ deg. n. following the 10th, towards *ω* Orionis. Conſiderably unequal. Both pr. Diſtance with 278, 13″ 40‴. Poſition 37° 3′ n. following.

53. *γ* Virginis borealior et ſequens.

Feb. 17, 1782. Double. Near 2½ degrees n. following *γ*, in a line parallel to *ε* and *α* Virginis; a conſiderable ſtar; a line from *γ* to this paſſes between two of nearly the ſame magnitude with this ſtar. A little unequal. Both d. Diſtance 12″ 58‴. Poſition 79° 0′ n. preceding.

III. 54. Secunda ad σ Urſæ majoris. FL. 13. In fronte.

June 2, 1782. Double. Extremely unequal. L. w.; S. r. Diſtance 7″ 56‴. Poſition 13° 0′ n. preceding.

55. υ (FL. 18am) Coronæ borealis ſequens ad boream.

June 14, 1782. Double. Conſiderably unequal. L. dr.; S. d. Diſtance with 227, about 3 or 4 diameters of L. being too obſcure for the micrometer. Poſition 53° 48′ f. preceding. Diſtance of the largeſt of the two from υ Coronæ 1′ 18″ 8‴. Poſition of the ſame with υ, 64° 24′ n. following.

56. S (FL. 72â) Serpentarii borealior.

June 16, 1782. Double. About 2½ degrees n. of the 72d Serpentarii; a conſiderable ſtar. A little unequal. Both r. Diſtance 7″ 37‴. Poſition 9° 42′ f. preceding. A third ſtar about 1′ preceding.

57. In Anſeris corpore.

Aug. 11, 1782. A pretty double ſtar. About ¾ degree n. of a cluſter of ſtars formed by the 4th, 5th, 7th, 9th Anſeris; in a line parallel to the 6th Vulpeculæ and β Cygni; that of two which is fartheſt from the cluſter. A little unequal. Both r. Diſtance 7″ 1‴. Poſition 58° 36′ f. following.

58. θ Perſei. FL. 13. In ſiniſtro humero.

Aug. 20, 1782. Double. Extremely unequal. L. w. inclining to r.; S. d. Diſtance with 932, 13″ 31‴. Poſition 20° 0′ n. preceding. A third ſtar, very unequal, within 1′; towards the ſouth.

59. Ad FL. 19am Perſei. In capite.

Aug. 20, 1782. Double. It is perhaps the 19th Perſei removed, or more likely a ſtar not marked in FLAMSTEED's Catalogue; the 19th being either vaniſhed, or miſplaced by

FLAMSTEED.

III. FLAMSTEED. Pretty unequal. L. bw.; S. br. Diſtance 12″ 2‴. Poſition 0° 0′ following.

60. Secunda ad *p* Perſei. FL. 20. Illas in larva præcedit.

Aug. 20, 1782. Double. Extremely unequal. L. rw.; S. d. Diſtance 14″ 2‴. Poſition 30° 30′ ſ. following.

61. Sub finem caudæ Draconis.

Aug. 29, 1782. Double. Of two conſiderable ſtars, about half-way between *α* and *ι* Draconis, that which is towards *ι*. The two ſtars are parallel to *ζ* and *ε* Urſæ majoris. Very unequal. L. pr.; S. db. Diſtance 12″ 30‴; perhaps a little inaccurate. Poſition 87° 42′ n. preceding.

62. FL. 35 Piſcium. In lino auſtrino.

Sept. 4, 1782. Double. Conſiderably unequal. L. rw.; S. pr. Diſtance 12″ 30‴. Poſition 58° 54′ ſ. following.

63. Prope FL. 65^{am} Sagittarii. Ad extremum paludamentum.

Sept. 5, 1782. Double. Near ½ degree ſ. following the 65th Sagittarii towards *ζ* Capricorni. Very unequal. Too low for colours; perhaps dw. Diſtance 14″ 20‴. Poſition 73° 48′ n. following.

64. FL. 26 Aurigæ. In dextri cruris involucro.

Sept. 5, 1782. Double. Very unequal. L. rw.; S. r. Diſtance 13″ 25‴. Poſition 2° 36′ n. preceding.

65. *e* (FL. 58^{a}) Perſei auſtralior. In dextri pedis talo.

Sept. 7, 1782. Double. About 10′ ſouth of the 58th Perſei, in a line parallel to *ζ* and *ι* Aurigæ; a ſmall teleſcopic ſtar. Very unequal. L. r.; S. d. Diſtance with 625, 11″ 22‴. Poſition 48° 54′ n. following. Very inaccurate: windy.

66. *e* Tauri. FL. 30. In dextri humeri ſcapula.

Sept. 7, 1782. Double. Extremely unequal. L. w.; S. r. Diſtance 11″ 16‴; inaccurate on account of obſcurity. Poſition 17° 15′ n. following.

III. 67. ι Leporis. FL. 3. Borea præcedentis lateris quadrilateri ad aures.

Sept. 7, 1782. Double. Exceſſively unequal. L. w.; S. d. With 227, there was not a poſſibility of meaſuring the diſtance, though the glaſs was carefully cleaned; on trying 625, I found the ſtar ſo ſtrong that it bore a very tolerable good light *. Diſtance with this power 12″ 20‴. Poſition 89° 21′ n. preceding.

68. η (FL. 17ª) Arietis auſtralior et præcedens.

Sept. 10, 1782. Double. Full 1 degree ſouth preceding η, in a line parallel to α and γ Arietis. Very unequal. L. pr.; S. d. Diſtance 8″ 5‴. Poſition 55° 42′ ſ. following.

69. Prope FL. 64ᵃᵐ Aquarii. In dextro femore.

Sept. 27, 1782. Double. Full 1½ degree n. following the 64th ::, in a line parallel to λ and φ Aquarii; the largeſt of two that follow a very obſcure triangle in the finder. Extremely unequal. L. rw.; S. db. Diſtance 12″ 46‴. Poſition 20° 3′ ſ. following.

70. x Cephei. FL. 1. In dextro crure.

Sept. 27, 1782. A beautiful double ſtar. Extremely unequal. L. fine w.; S. r. Diſtance 5″ 47‴. Poſition 32° 30′ ſ. following.

* With regard to ſmall ſtars, that become viſible by an increaſe of magnifying power, we may ſurmiſe, that it is partly owing to the greater darkneſs of the field of view, ariſing from the increaſed power, and partly to the real effect of the power; for, though the real diameter of a ſtar, notwithſtanding it be magnified a thouſand times, ſhould ſtill remain ſmaller than the minimum viſibile, yet ſince a ſtar of the ſeventh magnitude may be ſeen by the naked eye, we may conclude, that the light of a ſtar ſubtends incomparably a larger angle than its luminous body; and this may be in ſuch a proportion, with very ſmall ſtars, that the power of the teleſcope ſhall be juſt ſufficient to magnify the real diameter ſo as to bring it within the limits of this proportion, whereby the ſtar will become viſible.

III. 71. Tiaram Cephei præcedens.

Sept. 27, 1782. Treble. About 1½ degree preceding the *garnet ſtar* *, in a line parallel to ι and ζ Cephei. The two neareſt very unequal. L. w.; S. db. Diſtance 11″ 35‴. Poſition 35° 24′ f. following. The two fartheſt conſiderably unequal. S. db. Diſtance 18″ 37‴. Poſition 73° 57′ n. preceding. The place of the *garnet ſtar*, reduced to the time of FLAMSTEED's Catalogue, is about Æ 21 h. 45′. P.D. 32°½.

72. Tiaram Cephei præcedens.

Sept. 27, 1782. Double. Within ¼ degree of the foregoing treble ſtar. Conſiderably unequal. L. rw.; S. pr. Diſtance 13″ 7‴. Poſition 32° 0′ n. following.

73. FL. 25ª Ceti auſtralior et ſequens.

Oct. 2, 1782. Double. About ¾ degree f. following the 25th, in a line parallel to θ and τ Ceti. Pretty unequal. Diſtance with 278, 14″ 50‴. Poſition 89° 12′ f. preceding; perhaps a little inaccurate.

74. FL. 18ª Pegaſi auſtralior. Ad oculum ſiniſtrum.

Oct. 4, 1782. Double. About ¾ degree f. preceding the 18, in a line parallel to η and ε Pegaſi; the moſt north and largeſt of two. A little unequal. Both rw. Diſtance 14″ 29‴ full meaſure. Poſition 31° 33′ n. following.

75. Ad Genam Monocerotis.

Oct. 4, 1782. Double. About 1 degree n. of, and a little preceding the ſix teleſcopics in the place of the 12th, in a line parallel to the 12th Monocerotis and μ Geminorum.

76. τῶν quatuor teleſcopicarum, δ Orionis ſequentium, penultima.

Oct. 4, 1782. Double. About ¾ degree n. following δ, in a line parallel to τ and ι Orionis. Extremely unequal.

* Phil. Tranſ. vol. LXXIII. p. 257.

III. L. r.; S. d. Distance with 278, 9″ 12‴. Position 13° 6′ n. preceding.

77. FL. 65^{am} Arietis sequens ad austrum.

Oct. 9, 1782. Double. About ¾ degree s. following the 65th Arietis, in a line parallel to the Pleiades and ε Tauri; the preceding of two. Very unequal. L. r.; S. bluish. Distance 8″ 32‴. Position 73° 18′ s. following.

78. FL. 13^{am} Tauri præcedens ad austrum.

Oct. 9, 1782. Double. About 1¾ degree s. preceding the 13th Tauri, in a line parallel to ε Tauri and δ Ceti. Nearly equal. Both pr. Distance 7″ 10‴. Position 87° 57′ n. preceding.

79. ε (FL. 83^{a}) Ceti borealior.

Oct. 13, 1782. Double. About ⅔ degree n. of ε Ceti; the neareſt of three forming an arch. Extremely unequal. L. rw.; S. darkish red. Distance with 278, 10″ 48‴. Position 45° 12′ s. preceding.

80. σ (FL. 76^{am}) Ceti præcedens. In siniſtro crure.

Oct. 13, 1782. Double. Full 1½ degree preceding σ, towards τ Ceti. Extremely unequal. L. rw.; S. br. Distance 11″ 16‴. Position 22° 24′ n. preceding.

81. Parvula à ζ Lyræ ε versus.

Oct. 19, 1782. Double. Above ½ degree from ζ towards ε Lyræ. Extremely unequal. L. r.; S. dr. Distance 9″ 27‴ full measure. Position 66° 18′ n. following.

82. FL. 41 Aurigæ.

Nov. 6, 1782. A pretty double star. Considerably unequal. L. w.; S. grey inclining to r. Distance 8″ 32‴. Position 80° 0′ n. preceding.

III. 83. Fl. 19 Lyncis.

Nov. 13, 1782. Double. A little unequal. L. rw.; S. bw. Diſtance 14″ 11‴. Poſition 46° 54′ ſ. preceding.

84. Fl. 40 Lyncis. In Urſæ majoris pede.

Nov. 13, 1782. Double. Very or extremely unequal. L. wr.; S. r. Diſtance 7″ 11‴. Poſition 48° 12′ n. preceding.

85. Fl. 2 Canum Venaticorum.

Nov. 13, 1782. Double. Very unequal. L. r.; S. bluiſh. Diſtance 12″ 12‴. Poſition 11° 0′ ſ. preceding.

86. Fl. 57 Urſæ majoris.

Nov. 20, 1782. Double. The largeſt of two ſtars. Exceſſively unequal. L. w.; S. a red point without ſenſible magnitude. With 227, S. is but juſt viſible. Poſition 75° 36′ n. following.

87. Fl. 59^{a} Urſæ majoris borealior.

Nov. 20, 1782. A pretty treble ſtar. Near 1½ degree n. of the 59th, in a line parallel to ψ and β Urſæ majoris nearly. The two neareſt conſiderably unequal. L. pr.; S. r. Diſtance 12″ 30‴. Poſition 0° 0′ preceding. The two fartheſt very unequal. S. dr. Diſtance 32″ 21‴, Poſition 4° 0′ n. following.

88. Fl. 11^{a} Tauri borealior et ſequens.

Nov. 25, 1782. Double. About ½ degree n. following the 11th Tauri, towards ι Aurigæ. Very unequal. L. w.; S. pr. Diſtance with 278, 13″ 37‴. Poſition 89° 51′ n. following.

89. Ad 63^{am} Herculis. In linea per δ et ε ducta.

Nov. 26, 1782. Double. About 4 degrees from δ towards ε Herculis, near the 63d. Very unequal. L. r.; S. r. Diſtance 11″ 53‴. Poſition 47° 48′ n. following.

III. 90. FL. 103^{a} Tauri borealior.

Nov. 29, 1782. Double. About three degrees directly n. of the 103 Tauri; the largeſt of three, forming an obtuſe angle. Conſiderably unequal. L. rw.; S. pr. Diſtance with 278, 13″ 6‴. Poſition 64° 0′ n. following.

91. FL. 62^{a} Arietis borealior et ſequens,

Dec. 23, 1782. Double. Near 1 degree n. following the 62d Arietis, towards ε Perſei. Nearly equal. Both dw. Diſtance 11″ 17‴; not very accurate. Poſition 12° 24′ n. preceding or ſ. following.

92. ξ (FL. 77^{am}) Cancri præcedens ad boream.

Dec. 28, 1782. Double. About 1 degree n. preceding ξ Cancri, in a line parallel to ε Leonis and the 41ſt Lyncis; a conſiderable ſtar. A little unequal. Both rw. Diſtance 8″ 50‴. Poſition 65° 12′ ſ. preceding.

93. FL. 117 Tauri.

Dec. 31, 1782. Double. Almoſt equal. Both rw. Diſtance 12″ 12‴. Poſition 52° 27′ ſ. following.

94. ν (FL. 7^{am}) Leporis præcedens ad boream.

Dec. 31, 1782. Double. About 1 ⅓ degree n. preceding ν Leporis, in a line parallel to κ and ε Orionis; the ſecond in that line. Equal. Both rw. Diſtance 11″ 44‴. Poſition 4° 0′ ſ. following or n. preceding.

95. ν (FL. 48^{am}) Eridani præcedens ad auſtrum.

Jan. 2, 1783. Double. Near ⅓ degree ſ. preceding ν, in a line from the 51ſt continued through the 48th Eridani. Extremely unequal. L. rw.; S. d. and hardly to be ſeen with 227. Diſtance with 278, 15″ 21‴; very inaccurate on account of obſcurity. Poſition 9° 18′ ſ. preceding.

III. 96. Fl. 17 Crateris.

Jan. 10, 1783. Double. Nearly equal. Both rw. Distance 9″ 46‴. Position 64° 27′ f. preceding.

97. Fl. 54 Hydræ.

Jan. 10, 1783. Double. Very unequal. L. w.; S. bluish r. Distance 11″ 17‴; too low for great accuracy. Position 38° 15′ f. following.

98. Ad Genam Monocerotis.

Jan. 13, 1783. Double. About ⅔ degree f. preceding the most f. of a cluster of six telescopics in the place of the 12th, in a line parallel to the 15th and 12th Monocerotis. Excessively unequal. Position 61° 57′ f. preceding.

99. Fl. 55 Eridani.

Jan. 31, 1783. Double. A very little unequal. L. pr.; S. rw. Distance 9″ 9‴. Position 44° 9′ n. preceding.

100. Fl. 55am Eridani præcedens ad austrum.

Jan. 31, 1783. Double. About 2¼ degrees f. preceding the 55th Eridani, in a line parallel to Rigel and γ Eridani. Considerably unequal. L. pr.; S. db. Distance 11″ 53‴. Position 16° 24′ f. preceding.

101. *k* Centauri. Fl. 3.

Jan. 31, 1783. Double. Considerably unequal. L. dw.; S. dpr. Distance 11″ 35‴. Position 22° 0′ f. following.

102. *h* (Fl. 29am) Herculis præcedens ad austrum.

Feb. 3, 1783. Double. About 1¼ degree f. preceding *h* Herculis towards ε Serpentis; a small star. Very unequal. Both r. Distance 14″ 2‴. Position 67° 12′ n. following.

103. ε (Fl. 37â) Serpentis borealior et sequens.

March 4, 1783. Double. Near two degrees f. following ε, in a line parallel to the 13th Serpentis and 10th Serpentarii. Very unequal. L. pr.; S. r.; but a *dry fog*, if I may so

III. ſo call it, probably tinges them too deeply. Diſtance with 278, 12″ 34‴; with 625, 12″ 23‴. Poſition 50° 12′ n. preceding.

104. FL. 83ᵗᵐ Herculis præcedens.

Mar. 26, 1783. Double. About ⅓ degree preceding the 83; the ſecond ſtar towards the 79th Herculis. Very unequal. L. r.; S. darker r. Diſtance 14″ 20‴. Poſition 83° 48′ n. preceding.

105. γ (FL. 12ª) Sagittæ borealior et præcedens.

April 7, 1783. Double. About 2′ preceding the double ſtar V. 106. Pretty unequal. L. r.; S. d. Diſtance 14″ 29‴; very inaccurate, on account of obſcurity. Poſition 50° 24′ f. preceding.

106. FL. 5 Serpentis.

May 21, 1783. Double. Exceſſively unequal. L. rw.; S. db. Too obſcure for meaſures. Of the third claſs, far. Poſition about 30° or 40° n. following.

107. Congerie Stellularum Sagittarii borealior.

June 6, 1783. Double. Above 1¼ degree n. of the 20th cluſter of ſtars of the *Connoiſſance des Temps*, in a line parallel to γ Sagittarii and the cluſter: the moſt ſouth of many. Conſiderably unequal. Diſtance with 278, 15″ 10‴. As accurate as the priſmatic power of the atmoſphere, which lengthens the ſtars, will permit. Poſition 54° 48′ f. preceding *.

108.

* What I call the priſmatic power of the atmoſphere, of which little notice has been taken by aſtronomers, is that part of its refractive quality whereby it diſperſes the rays of light, and gives a lengthened and coloured image of a lucid point. It is very viſible in low ſtars; FOMALHAND, for inſtance, affords a beautiful priſmatic ſpectrum. That this power ought not to be overlooked in delicate

III. 108. Fl. 19^{am} Aquilæ præcedens ad boream.

July 7, 1783. Double. Above ¾ n. preceding the 19th, in a line parallel to β and ζ Aquilæ. Very unequal. L. r..; S. dr. Distance 12″ 58‴. Position 58° 27′ f. following.

109. Fl. 19^{am} Aquilæ præcedens ad Boream.

July 7, 1783. Double. About 1⅓ degree n. preceding the 19th, in a line parallel to ε and δ Aquilæ. Pretty unequal. Both rw. Distance 10″ 13‴. Position 22° 6′ n. preceding.

110. Fl. 77^{a} Cygni borealior et præcedens.

Sept. 17, 1783. Quadruple. Full ¾ degree n. preceding the 17th, in a line parallel to σ and α Cygni; a small star. The two nearest extremely unequal. L. r.; S. d. Distance with 625, 13″ 54‴. Position 67° 36′ f. following. The two largest a very little unequal. Both r. Distance with 278, 25″ 58‴. Position 40° 33′ n. following.

and low observations, is evident from some measures I have taken to ascertain its quantity. Thus I found, May 4, 1783, that the perpendicular diameter of ε, Flamsteed's 20th Sagittarii, measured 16″ 9‴, while the horizontal was 8″ 35‴; which gives 7″ 34‴ for the prismatic effect: the measures were taken with 460, near the meridian, and the air remarkably clear. And though this power, which depends on the obliquity of the incident ray, diminishes very fast in greater altitudes, yet I have found its effects perceivable as high, not only as α or γ Corvi in the meridian, but up to Spica Virginis, and even to Regulus. Experiments on these two latter stars I made November 20, 1782; when Regulus, at the altitude of 49°, shewed the purple rather fuller at the bottom of the field of view than when it was at the upper edge; which shews that the prismatic powers of the edges of the eye lens were assisted in one situation by the power of the atmosphere, but counteracted by it in the other. I turned the eye lens in all situations, to convince myself that it was not in fault. This experiment explains also, why a star is not always best in the center of the field of view; a fact I have often noticed before I knew the cause.

III. lowing. The fartheſt very unequal. S. d. Poſition almoſt in a line with the two largeſt.

111. ε (FL. 46^{a}) Orionis borealior et ſequens.

Sept. 20, 1783. Treble. About 1¼ degree n. following ε, towards α Orionis. The two neareſt of the third claſs.

112. δ (FL. 18^{am}) Cygni ſequens ad auſtrum.

Sept. 22, 1783. Double. About 1 degree ſ. following δ, towards the 47th Cygni; a pretty conſiderable ſtar. Equal, or perhaps the ſouthern ſtar the ſmalleſt. Both pr. Diſtance with 278, 10″ 8‴. Poſition 71° 0′. ſ. following.

113. FL. 27^{am} Cygni præcedens ad auſtrum.

Sept. 23, 1783. Quadruple and Sextuple. About ½ degree ſ. preceding the treble ſtar I. 96.; the middle of three, the moſt north whereof is the 27th Cygni. In the quadruple or n. preceding ſet, the two neareſt very unequal. Diſtance with 278, 11″ 16‴. Poſition 26° 0′ n. preceding; the two largeſt almoſt equal. Both r. Diſtance with 278, 29″ 27‴. Poſition 57° 12′ n. following. In the ſextuple or ſ. following ſet, the two largeſt pretty unequal. Both r. Diſtance with 278, 19″ 20‴. Poſition 27° 36′ ſ. preceding. All the other ſtars are as ſmall as the ſmalleſt of the quadruple ſet, and ſome of them much ſmaller.

114. FL. 16^{am} Monocerotis præcedens ad boream.

Jan. 23, 1784. Double. About 1¼ degree n. preceding the 16th.

FOURTH CLASS OF DOUBLE STARS.

IV. 45. In pectoris crate Orionis.

Dec. 27, 1781. Double. About $\frac{2}{3}$ degree following ψ, towards n Orionis. Extremely unequal. L. pr.; S. dr. Diſtance with 278, 20″ 3″. Poſition 62° 24′ f. following.

46. Fl. 21 :: Geminorum *.

Dec. 27, 1781. Double. A little unequal. Both pr. Diſtance about 25″. Poſition

47. Fl. 3 Leonis.

Feb. 2, 1782. Double. Exceſſively unequal. L. r.; S. d.; not viſible with 227. Diſtance eſtimated with 460, about 24″. Poſition a little n. following. A third ſtar in view. Diſtance perhaps 2′. Poſition about 15° f. following.

48. H (Fl. 1am) Geminorum præcedens ad boream.

Feb. 6, 1782. Quintuple. In the form of a croſs. About $\frac{2}{3}$ degree n. preceding H Geminorum, in a line parallel to the 65th Orionis and ζ Tauri; the middle of three. The two neareſt or preceding of the five extremely unequal. Diſtance 20″ 57‴. Poſition 7° 27′ f. preceding. The laſt of the three, in the ſhort bar of the croſs, has an exceſſively obſcure ſtar near it of the third claſs. Five more in view, differently diſperſed about the quintuple.

* The 21ſt and 20th Geminorum are not in the heavens as they are marked in Flamsteed's Atlas, ſo that it becomes doubtful whether the N° 21. is right.

IV. 49. ξ (FL. 4^{am}) Virginis ſequens ad boream.

Feb. 6, 1782. Double. 1 full degree n. following ξ Virginis, in a line parallel to ι and β Leonis. A little unequal. L. pr.; S. dr. Diſtance 27″ 28‴. Poſition 56° 30′ f. preceding.

50. FL. 17 Virginis. In pectore.

Feb. 6, 1782. Double. Conſiderably unequal. L. w.; S. bluiſh. Diſtance 20″ 9‴. Poſition 58° 21′ n. preceding.

51. *k* Virginis :: FL. 44 :: †. In ala auſtrina.

Feb. 6, 1782. Double. A ſtar ſouth of three forming an arch, and of the ſame magnitude with the middle one of the arch. Extremely unequal. L. w.; S. db. Diſtance 22″ 17‴; inaccurate. Poſition 32° 30′ n. following.

52. * ι Cancri. FL. 48. In boreali forfice.

Feb. 8, 1782. Double. Conſiderably unequal. L. rw.; S. d. garnet. Diſtance 29″ 54‴. Poſition 39° 54′ n. preceding; a little inaccurate.

53. π Geminorum. FL. 80. Supra capita.

Feb. 9, 1782. Double. Exceſſively unequal. L. garnet; S. d. Diſtance with 460, 21″ 30‴. Poſition

Other very ſmall ſtars in view.

54. δ (FL. 4^{am}) Hydræ ſequens.

Feb. 11, 1782. Double. About $\frac{1}{2}$ degree following δ, towards ζ Hydræ. Pretty unequal. Both pr. S. deeper. Diſtance 25″ 43‴. Poſition 59° 24′ n. following.

55. FL. 41^{am} Lyncis ſequens. In caudæ fine.

Mar. 5, 1782. Double. About $3\frac{1}{2}$ minutes n. following the 41ſt Lyncis. Extremely unequal. L. r.; S. dr. Diſtance 15″ 52‴; a little inaccurate. Poſition 50° 48′ n. preceding; inaccurate.

† Perhaps the 45th; requires fixed inſtruments to determine.

56.

IV. 56. Fl. 18 Libræ.

April 3, 1782. Double. The following of two. Extremely unequal. L. r.; S. b. Diſtance 17″ 59‴. Poſition 44° 45′ n. following.

57. Fl. 42am Comæ Berenices ſequens ad auſtrum.

April 15, 1782. Double. About 3 degrees ſ. following the 42d Comæ Berenices towards υ Bootis; the vertex of an iſoſceles triangle. Extremely unequal. Diſtance with 625, 16″ 42‴. Poſition 46° 31′ ſ. preceding.

58. Fl. 36am Comæ Berenices præcedens ad boream.

April 18, 1782. A pretty double ſtar. About 2½ degrees n. preceding the 36th, in a line parallel to the 42d and 15th Comæ Berenices; the following of two unequal ſtars. A little unequal. Both rw. Diſtance 15″ 52‴. Poſition 67° 57′ ſ. preceding.

59. Prope α Lyræ.

May 12, 1782. Double. About 2 or 3 minutes ſ. preceding α Lyræ. Very unequal. Both d. Diſtance with 278, 22″ 20‴. Poſition 33° 57′ n. preceding. Poſition of the largeſt with regard to α Lyræ 59° 12′ ſ. preceding.

60. Fl. 4am Urſæ majoris ſequens ad boream.

June 6, 1782. Double. Near 1 degree n. following the 4th, in a line parallel to *o* and *h* Urſæ majoris; a pretty large ſtar. Extremely unequal. L. r.; S. d. Diſtance near 30″; but too obſcure for meaſures.

61. ζ (Fl. 7â) Coronæ auſtralior et præcedens.

July 18, 1782. Double. Near ½ degree ſ. preceding ζ, towards η Coronæ bor. Nearly equal. Both pr. Diſtance 16″ 46‴. Poſition 4° 57′ n. following.

62.

IV. 62. τ (FL. 22^{a}) Herculis auſtralior et ſequens.

Aug. 11, 1782. Double. About $2\frac{1}{2}$ degrees ſ. following τ Herculis, in a line parallel to ι and γ Draconis; a conſiderable ſtar. Very or extremely unequal. L. w.; S. br. Diſtance $16''\ 51'''$. Poſition $72^{\circ}\ 15'$ ſ. preceding.

63. FL. 42 Herculis. Dextrum ſupra genu.

Aug. 11, 1782. Double. Very unequal. L. r.; S. rw. Diſtance $21''\ 31'''$. Poſition $3^{\circ}\ 42'$ ſ. following.

64. Prope q (FL. 12^{am}) Perſei.

Aug. 20, 1782. Double. Within a few minutes of q Perſei. Pretty unequal. Both pr.; but S. a little darker. Diſtance $21''\ 59'''$. Poſition $57^{\circ}\ 57'$ ſ. preceding.

65. Prope FL. 3^{am} Caſſiopeiæ.

Aug. 25, 1782. Double. Within 10 minutes of the 3d Caſſiopeiæ. Very unequal. L. pr.; S. r. Diſtance $20''\ 46'''$; very inaccurate. Poſition $41^{\circ}\ 12'$ ſ. following.

66. θ (FL. 33^{am}) Caſſiopeiæ præcedens.

Aug. 28, 1782. Double. About $1\frac{3}{4}$ degree ſ. of, and a little preceding θ, in a line from δ continued through θ Caſſiopeiæ. Extremely unequal. L. r.; S. db. Diſtance $24''\ 2'''$; very inaccurate. Poſition 13° 12. n. following; inaccurate.

67. † FL. 40 et 41 Draconis.

Aug. 29, 1782. Double. A little unequal. L. rw.; S. pr. Diſtance $20'\ 39'''$ mean meaſure; very accurate. Poſition $35^{\circ}\ 15'$ ſ. preceding *. There is a third, much ſmaller ſtar. Diſtance $3'\ 16''\ 33'''$. Poſition about 30° ſ. following.

* The proper motion of one of theſe ſtars at leaſt ſince the time of FLAMSTEED is evident, as he gives us their difference in AR 2′, and in PD 3′ 5″. Poſition ſ. preceding. Hence we have the hypotenuſe or diſtance above 3′ 40″, inſtead of $20''\ 39'''$; and the angle $86^{\circ}\ 17'$ inſtead of $35^{\circ}\ 15'$.

IV. 68. Fl. 77 Piſcium. In lini flexu.

Sept. 3, 1782. Double. A little unequal. L. wr.; S. pr. Diſtance 29″ 36‴. Poſition 4° 48′ n. following. In both meaſures the weather too windy for accuracy.

69. Fl. 23^{am} Andromedæ præcedens.

Sept. 4, 1782. Double. Full 1½ degree preceding the 23d, in a line parallel to ν and ι Andromedæ. Of two double ſtars in the finder the largeſt of the preceding ſet. Very unequal. L. r.; S. d. Diſtance with 278, 21″ 58‴. Poſition 70° 36′ n. preceding.

70. Fl. 51 Piſcium. In auſtrino lino.

Sept. 4, 1782. Double. Very unequal. L. rw.; S. d. Diſtance with 278, 22″ 29‴. Poſition 0° 36′ n. following.

71. * *o* Capricorni. Fl. 12. Trium in roſtro auſtrina.

Sept. 5, 1782. Double. Pretty unequal. Both rw. Diſtance 23″ 30‴. Poſition 30° 45′ ſ. preceding.

72. Fl. 55^{a} Perſei borealior.

Sept. 7, 1782. Double. About ¼ degree n. of the 55th Perſei; of three in a line the moſt north. Pretty unequal. L. rw.; S. pr. Diſtance with 278, 16″ 51‴. Poſition 27° 24′ n. following.

73. In Conſtellatione Camelopardali.

Sept. 7, 1782. Double. Between Fl. 2 and 8 Cam.; the ſmalleſt of two that are within ¼ degree of each other. Conſiderably unequal. Diſtance 19″ 32‴. Poſition 85° 0′ ſ. preceding.

74. δ (Fl. 68^{am}) Tauri ſequens ad boream.

Sept. 7, 1782. Double. Near ½ degree n. following δ, towards ι. Tauri. Very unequal. L. pr.; S. r. Diſtance 16″ 31‴. Poſition 25° 45′ n. following.

75.

IV. 75. *r* (FL. 66^{am}) Tauri ſequens.

Sept. 7, 1782. Double. About 1¼ degree n. following *r*, in a line parallel to *μ* Tauri and the 9th Orionis. Very unequal. L. r.; S. dr. Diſtance 22″ 35‴. Poſition 61° 36′ ſ. following.

76. FL. 13^{am} Ceti præcedens ad auſtrum.

Sept. 9, 1782. Double. About 1 degree ſ. preceding the 13th, towards the 8th Ceti. Conſiderably unequal. L. rw.; S. br. Diſtance with 278, 18″ 35‴. Poſition 40° 24′ n. following.

77. FL. 37^{a} Ceti borealior. In dorſo.

Sept. 22, 1782. Double. About ¼ degree n. preceding the 37th, towards the 36th Ceti. Very unequal. L. r.; S. dr. Diſtance 19″ 6‴. Poſition 63° 24′ n. preceding.

78. *η* (FL. 3^{am}) Cephei præcedens.

Sept. 27, 1782. Double. About 1¼ degree preceding *η*, in a line from *ε* continued through *η* Cephei. Very unequal. L. r.; S. d. Diſtance 19″ 32‴. Poſition 40° 36′ n. following.

79. *μ* Cephei. FL. 13. Ad coronam.

Sept. 27, 1782. Double. A little unequal. L. w.; S. rw. Diſtance 21″ 3‴. Poſition 77° 48′ ſ. preceding.

80. *β* (FL. 2^{a}) Canis majoris borealior.

Sept. 30, 1782. Double. About 1¾ degree n. of *β* Canis majoris towards the 11th Monocerotis; the moſt n. of two. Conſiderably unequal. Diſtance 17″ 59‴; difficult to take, and perhaps a little inaccurate. Poſition 2° 24′ n. following.

81. *ν* Canis majoris. FL. 6. In dextro genu.

Sept. 30, 1782. Double. Conſiderably unequal. L. rw.; S. pr. Diſt. 18″ 19‴. Poſition very near directly preceding.

IV. 82. Prope Fl. 16am Cephei. In cingulo.

Sept. 30, 1782. Double. Above ¼ degree following the 16th Cephei, in a line parallel to β and α Caſſiopeiæ. Conſiderably unequal. L. orange. S. r. Diſtance 28″ 5‴. Poſition 79° 18′ n. preceding.

83. Fl. 26 Ceti. Supra dorſum.

Oct. 2, 1782. Double. Very unequal. L. rw. S. db. Diſtance 17″ 2‴ mean meaſure. Poſition 14° 36′ f. preceding.

84. *m* Orionis. Fl. 23 In crate pectoris

Oct. 2, 1782. Double. Conſiderably unequal. L. w.; S. pr. Diſtance with 278, 26″ 9‴. Poſition 59° 33′ n. following.

85. Fl. ultima Lacertæ.

Oct. 4, 1782. Treble. The two neareſt extremely unequal. L. rw.; S. d. Diſtance 20″ 27‴. Poſition 79° 33′ n. preceding. The next very unequal; S. r. Diſtance 54″ 57‴; inaccurate. Poſition 44° 24′ n. following. A fourth and fifth ſtar in view.

86. Fl. 8 Lacertæ. In media cauda.

Oct. 4, 1782. Quadruple. The two largeſt and neareſt a little unequal. Both rw. Diſtance 17″ 14‴. Poſition 84° 30′ f. preceding. The two next very unequal, of the fourth claſs. The two remaining conſiderably unequal, of the fifth claſs. They form an arch.

87. *e* (Fl. 29am) Orionis præcedens. In ſiniſtro calcaneo.

Oct. 4, 1782. Double. About 1 degree preceding *e*, in a line parallel to σ Orionis and *b* Eridani nearly. Conſiderably unequal. Both pr. Diſtance 29″ 18‴. Poſition 82° 18′ n. following.

IV. 88. Fl. 7 Tauri. In dorso.

Oct. 9, 1782. Double. Very unequal. L. pr.; S. dr. Distance 19″ 50‴. Position 23° 15′ n. following.

89. E telescopicis caudam Arietis sequentibus.

Oct 9, 1782. Double. The vertex of an isosceles triangle following τ Arietis; a very small star. Very unequal. L. r.; S. d. Distance with 278, 20″ 3‴. Position 62° 0′ s. following.

90. Ad Fl. 18am Ursæ minoris. Prope eductionem caudæ.

Oct. 12, 1782. Double. The largest of six or seven stars, and most south of a triangle formed by three of them. A little unequal. L. pr.; S. deeper pr. Distance 26″ 24‴. Position 3° 12′ n. following.

91. Fl. 2 Navis.

Oct. 12, 1782. A pretty double star. A little unequal. L. w.; S. w. inclining to r. Distance 17″ 23‴. Position 69° 12′ n. preceding.

92. β inter et ζ Delphini.

Oct. 17, 1782. Treble. Between β and ζ, but nearer to β Delphini. All three nearly equal. All wr. Distance of the two nearest with 278, 21″ 33‴. Position 118° 27′ n. preceding.

93. ε (Fl. 4am) Lyræ sequens.

Oct. 19, 1782. Double. About 3 degrees following ε, in a line parallel to α and θ Lyræ; the largest of two. Extremely unequal. L. w.; S. r. Distance 19″ 50‴. Position 24° 0′ s. preceding.

94. E borealibus telescopicis β Lyræ præcedentibus.

Oct. 19, 1782. Double. Full 2 degrees n. preceding β Lyræ, in a line parallel to the 18th and ε; the sixth telescopic star.

Considerably

IV. Conſiderably unequal. L. rw.; S. pr. Diſtance 22″ 53‴. Poſition 5° 24′ n. following.

95. Fl. 25am Monocerotis præcedens.

Oct. 19, 1782. Quadruple. About 2½ degrees preceding, and a little n. of the 25th Monocerotis. Two large ſtars always to be ſeen, and two more only viſible in dark nights. The neareſt which is that to the ſmalleſt of the two large ones, extremely unequal. Diſtance 20″ 27‴. Poſition following.

96. Fl. 25am Monocerotis ſequens. In latere.

Oct. 19, 1782. Double. About 1¼ n. following the 25th, in a line parallel to the 21ſt Monocerotis and Procyon. A little unequal. Both dr. Diſtance 18″ 19‴. Poſition 24° 0′ ſ. preceding.

97. Fl. 29 Monocerotis. In femore.

Oct. 19, 1782. Double. Extremely unequal. L. wr.; S. d. Diſtance 29″ 54‴. Poſition 15° 12′ ſ. following. Six more in view.

98. α (Fl. 58am) Orionis ad auſtrum præiens.

Oct. 29, 1782. Double. About ½ degree preceding α, towards ζ Orionis. Equal. Both r. Diſtance 17″ 59‴; a little inaccurate.

99. Duarum teleſcopicarum δ Sagittæ ad auſtrum ſequentium borea.

Nov. 6, 1782. Treble. Of a trapezium, conſiſting of this treble ſtar, δ, ζ, and the 9th Sagittæ, it is the corner oppoſite to ζ; the neareſt to ζ of two. The two neareſt very unequal. L. pr.; S. db. Diſtance 21″ 22‴; inaccurate. Poſition 0° 0′ following. The two largeſt a little unequal; of the fifth claſs. Poſition 10° 36′ ſ. preceding.

IV. 100. χ Sagittæ. Fl. 13. Infra mediam arundinem.
Nov. 6, 1782. Treble. The largeſt of three. The two neareſt equal. Both r. Diſtance 23″ 2‴. Poſition 10° 12′ f. preceding. The third is a large ſtar. Diſtance above 1 minute. Poſition about 10° or 15° n. preceding the other two.

101. φ (Fl. 24^{a}) Aurigæ borealior et præcedens.
Nov. 6, 1782. Double. Near $\frac{3}{4}$ degree n. preceding φ, in a line parallel to the 21ſt and 8th Aurigæ. Pretty unequal. L. rw. S. bluiſh. Diſtance 25″ 29‴. Poſition 76° 0′ n. preceding.

102. Fl. 59 Aurigæ.
Nov. 6, 1782. Double. The apex of an iſoſceles triangle. Very or extremely unequal. L. rw.; S. Diſtance 23″ 30‴. Poſition 50° 3′ f. preceding.

103. Fl. 77^{am} Draconis ſequitur.
Nov. 13, 1782. Double. Near $\frac{3}{4}$ degree following the 77th Draconis, in a line parallel to $\varkappa$ Cephei and the 76th Draconis nearly; of a rectangular triangle the leg neareſt the 77th. Very unequal. L. r.; S. bluiſh r. Diſtance 22″ 35‴. Poſition 45° 48′ n. following.

104. Inter γ et 55^{am} Andromedæ.
Nov. 13, 1782. Double. A little more than 1 degree n. following the 55th Andromedæ, in a line parallel to β Trianguli and Algol. Conſiderably unequal. L. r.; S. d. Diſtance with 278, 18″ 57‴. Poſition 22° 33′ n. following.

105. δ Corvi. Fl. 7. Duarum in ala ſequente præcedens.
Nov. 13, 1782. Double. Extremely unequal. L. w.; S. r. Diſtance 23″ 30‴. Poſition 54° 0′ f. preceding.

106.

IV. 106. α (Fl. 50^{am}) Urſæ majoris ſequens ad boream.

Nov. 17, 1782. Double. About $1\frac{3}{4}$ degree n. following α, in a line parallel to β Urſæ et $\varkappa$ Draconis; the laſt of three in a row. Extremely unequal. Both r. Diſtance 18″ 55‴; very inaccurate. Poſition 44° 33′ ſ. following. A third ſmall ſtar in view.

107. Fl. 79^{a} Pegaſi auſtralior et præcedens.

Nov. 20, 1782. Double. About $\frac{3}{4}$ degree ſ. preceding the 79th, towards π Pegaſi; at the center of a trefoil. Very unequal. L. r.; S. d. Diſtance with 278, 26″ 12‴. Poſition 50° 21′ n. following.

108. Fl. 69^{a} Urſæ majoris auſtralior.

Nov. 20, 1782. Double. Near 2 degrees ſ. of the 69th, towards the 63d Urſæ majoris. A very little unequal. Both r. Diſtance 19″ 15‴; very inaccurate. Poſition 10° 12′ n. following.

109. Fl. 62 Tauri.

Nov. 25, 1782. Double. Conſiderably unequal. L. w.; S. r. Diſtance 28″ 5‴. Poſition 21° 12′ n. preceding.

110. β (Fl. 112^{a}) Tauri borealior et ſequens.

Dec. 24, 1782. Double. About $1\frac{1}{4}$ degree n. following β Tauri, towards θ Aurigæ; the ſecond in that direction. Very unequal. L. r.; S. d. Diſtance 16″ 1‴. Poſition 74° 54′ n. preceding.

111. Fl. 54 Cancri.

Dec. 28, 1782. Double. A little unequal. Both rw. S. a little darker. Diſtance 17″ 14‴. Poſition 29° 0′ ſ. following.

112. γ (Fl. 15^{am}) Crateris ſequens ad boream.

Jan. 1, 1783. Double. About 1 degree n. following γ Crateris, in a line parallel to δ Corvi and Spica. Equal. Both pr.

Diſtance

IV. Distance 26″ 15‴; too low for accuracy. Position 58° 42′ n. preceding or s. following.

113. Fl. 61^{a} Cygni borealior et præcedens.

Jan. 6, 1783. Double. About 1¼ degree n. preceding the 61st, in a line parallel to υ and α Cygni. Very or extremely unequal. L. r.; S. db. Distance with 278, 17″ 30‴. Position 28° 24′ n. preceding. A third star in view.

114. *t* (Fl. 12^{a}) Virginis australior.

Jan. 8, 1783. Double. About 1½ degree s. of *t* Virginis. Very unequal. L. pr.; S. d. Distance 23″ 21‴. Position 15° 54′ n. preceding.

115. φ (Fl. 11^{am}) Herculis præcedens ad austrum.

Jan. 10, 1783. Double. About 2½ degrees s. of, and a little preceding φ, in a line parallel to η and ζ Herculis; the largest of three or four. Extremely unequal. L. r.; S. b. Distance 20″ 54‴. Position 43° 48′ n. following.

116*. Fl. 83^{am} Pegasi sequens ad boream.

Jan. 13, 1783. Double. Equal. Both w. Distance 28″ 59‴. Position 68° 21′. Mr. C. MAYER, in 1777, settled its place AR 0^{h}. 52′ 53″ in time, and 20° 17′ 53″ in declination N.

117. Fl. 42^{a} Eridani australior.

Jan. 31, 1783. Double. About 1¼ degree s. of the 42d Eridani, in a line parallel to Rigel and μ Leporis; the most south and following of three. Very unequal. L. r.; S. r. Distance 19″ 32‴. Position 31° 48′ s. preceding.

118. ι (Fl. 48^{am}) Cancri sequens.

Feb. 5, 1783. Double. Full ½ degree following the 48th, in a line parallel to δ Cancri and ε Leonis; a very small star, next to two more which are nearer to ι. A little une-

IV. qual. Distance 24″ 6‴. Position about 25° n. following.

119. ι (Fl. 68^{am}) Virginis præcedens ad austrum.

Feb. 7, 1783. Double. About 1 degree s. preceding the 68th, in a line parallel to the 99th and α Virginis. Extremely unequal. Distance 21″ 49‴. Position 36° 54′ n. preceding.

120. Fl. 82^{am} Piscium sequens ad boream.

Feb. 27, 1783. Double. About ¼ degree n. following the 82d Piscium, in a line parallel to α and β Trianguli; the largest of two. Considerably unequal. L. rw.; S pr. Distance 18″ 19‴. Position 21° 0′ s. preceding. A third star in view.

121. σ Scorpii Fl. 20 præcedens trium lucidarum in corpore.

Mar. 1, 1783. Double. Very unequal. L. whitish; S. r. Distance 21″ 40‴. Position 0° 0′ (or perhaps 1°) n. preceding.

122. Fl. 32^{a} Ophiuchi borealior et præcedens.

Mar. 7, 1783. Double. Near 1 degree n. of, and a little preceding the 32d Ophiuchi, in a line parallel to α and η Herculis. Very unequal. Distance 21″ 3‴. Position 25° 3 s. preceding.

123. Fl. 19 Ophiuchi.

Mar. 9, 1783. Double. The most south of two. Very unequal. L. pr.; S. d. Distance 20″ 27‴. Position 3° 9′ s. following.

124. ψ (Fl. 4^{am}) Ophiudhi præcedens ad austrum.

Mar. 24, 1783. Double. About ½ degree preceding and a little s. of ψ, in a line parallel to ψ Ophiuchi and ω Scorpii; in the base of a triangle, the nearest to ψ. A little unequal.

IV. Both inclining to l r. Distance 15″ 24‴. Position 62° 54′ n. following.

125. Fl. 29 Camelopardali.

April 2, 1783. Double. Very unequal. L. pr.; S. d. Distance 22″ 26‴; very inaccurate. Position 47° 36′. f. following; a little inaccurate.

126. λ (Fl. 22ª) Cephei borealior et præcedens.

April 20, 1783. Double. Less than ½ degree n. preceding λ, in a line almost parallel to δ and ζ Cephei; a considerable star. A little unequal. Both dw. Distance 18″ 50‴. Position 45° 39′ n. preceding.

127 †. λ (Fl. 16ªm) Aquilæ sequens ad boream.

May 21, 1783. Double. About 2½ degrees n. following the farthest of two which are about 1½ degree from λ, in a line parallel to λ and δ Aquilæ. Very unequal. L. rw.; S. dr. Distance 17″ 14‴; more exact with 932, 15″ 52‴. Position 69° 54′. n. preceding. Mr. PIGOTT, who favoured me with it, gives its place Æ. 18ʰ 52′½ ±, Declination 1° 0′ S.

128. γ (Fl. 57ªm) Andromedæ præcedens ad austrum.

July 28, 1783. Double. About 1⅓ degree f. preceding γ almost towards β Andromedæ; more exact towards σ Piscium; one not in a row of stars which are near that place. Considerably unequal. L. pr.; S. dr. Distance 15″ 42‴. Position 24° 12′ n. following.

129. Fl. 59 Andromedæ.

July 28, 1783. Double. A little unequal. L. rw.; S. pr. Distance 15″ 15‴. Position 55° 9′ n. following. A third star in view about 58° or 60° f. preceding.

IV. 130. η (Fl. 99^{i}) Piſcium borealior et ſequens.

Aug. 2, 1783. Double. About 1½ degree n. of, and a little following η Piſcium, in a line parallel to β Arietis and β Trianguli; the laſt of four in a crooked row. Very unequal. L. r.; S. darker r. Diſtance with 278, 15″ 49‴. Poſition 62° 15′ n. following.

131. Fl. 100 Piſcium.

Aug. 2, 1783. Double. Pretty unequal. L. pr.; S. r. Diſtance 15″ 52‴. Poſition 5° 0′ n. following.

132. Fl. 46^{am} Aquilæ ſequens ad boream.

Aug. 6, 1783. Double. About ½ degree n. following 46 Aquilæ, in a line parallel to α and γ Sagittæ. Very unequal. L. r.; S. db. Diſtance 22″ 44‴. Poſition 41° 24′ n. preceding.

FIFTH CLASS OF DOUBLE STARS.

V. 52. Secunda a ν Geminorum μ verſus.

Dec. 27, 1781. Double. The ſecond ſtar from ν towards μ Geminorum. Pretty unequal. L. r.; S. b. Diſtance 35″; inaccurate.

53. p Geminorum. Fl. 63. In inguine ſequentis II^{i}.

Dec. 27, 1781. Double. The brighteſt of two. Extremely unequal. L. pr.; S. d. Diſtance 44″ 15‴.

54. θ Hydræ. Fl. 22. Duarum in eductione cervicis ſequens.

Jan. 20, 1782. Double. Exceſſively unequal. L. w.; S. a point. Diſtance near 1 minute, too obſcure for meaſures, and

V. not visible till after having looked a good while at it. Position about 75° s. following.

55. Ad Fl. 12am Geminorum. In pede II^{i} præcedentis siniſtro.

Jan. 30, 1782. Treble. A ſmall ſtar near the place of the 12th Geminorum. The two neareſt a little unequal. Diſtance leſs than 1′.

56. Fl. 15 Geminorum. Dextrum prioris II^{i} pedem attingens.

Jan. 30, 1782. Double. Conſiderably or very unequal. L. r.; S. d. Diſtance 32″ 39‴. Poſition near 60° s. preceding.

57. Fl. 9â Orionis borealior et ſequens. In exuviarum ſummo.

Feb. 4, 1782. Treble. More than 1 degree n. following the 9th Orionis, towards the 113th Tauri; the largeſt of two. The two neareſt conſiderably unequal. L. rw.; S. rw. Diſtance with 278, 36″ 26‴. Poſition 33° 36′. The fartheſt very unequal. S. r. Diſtance Vth Claſs. Poſition following.

58. Fl. 7 Leonis. Supra pedem borealem anteriorem.

Feb. 4, 1782. Double. Very unequal. L. rw.; S. r. Diſtance 42″ 25‴. Poſition 8° 36′ n. following.

59. θ Cancri. Fl. 31. In quadrilatero circa Nubem.

Feb. 6, 1782. Double. Extremely unequal. L. r.; S. d. Diſtance 44″ 52‴. Poſition n. following.

60. *o* (Fl. 95am) Leonis præcedens; ad caudam.

Feb. 9, 1782. Double. Near ¾ degree s. preceding the 95th, in a line parallel to β and ϱ Leonis. Very unequal. L. rw.; S. d. Diſtance 37″ 15‴. Poſition 70° 48′ n. following.

61. Fl. 81 Leonis. In clune.

Feb. 9, 1782. Double. Extremely unequal. L. rw.; S. r. Diſtance 57″ 23‴. Poſition

V. 62. Fl. 57 Leonis. E posteriores pedes præcedentibus.

Feb. 11, 1782. Double. Very unequal. Distance 33″ 16‴.

63. Fl. 25 Leonis. In infimo pectore.

Feb. 17, 1782. Double. The largest of two. Extremely unequal. L. pr.; S. d. Distance 52″ 46‴. Position

64. Fl. 43ª Leonis australior. Ad sinistrum anteriorem cubitum.

Feb. 17, 1782. Double. Near 1 degree s. of the 43d, in a line parallel to η and α Leonis. Extremely unequal. L. w. inclining to r.; S. db. Distance 59″ 40‴. Position

65. Secunda ad π Canis majoris. Fl. 17. In pectore.

Mar. 3, 1782. Treble. The two nearest very unequal. L. rw.; S. r. Distance 44″ 52‴. Position 64° 12′ s. following. The two farthest very or extremely unequal. S. r. Distance Vth Class. Position about 85° s. preceding. The three stars form a rectangle, the hypotenuse of which contains the largest and smallest.

66. p (Fl. 63ª) Geminorum borealior.

Mar. 3, 1782. Double. About $\frac{3}{4}$ degree n. of, and a little preceding p, in a line parallel to υ and α Geminorum. Very unequal. L. pr.; S. d. Distance 34″ 39‴. Position 1° or 2° n. preceding.

67. Pollucem prope. In capite sequentis IIⁱ.

Mar. 3, 1782. Double. Near 1 degree n. following β, in a line from δ continued through β Geminorum nearly; the farthest and smallest of three. Considerably unequal. L. r.; S. dr. Distance 47″ 37‴.

68. Fl. 75ᵃᵐ Leonis præcedens ad boream.

Mar. 5, 1782. Treble. One of two n. preceding the 75th, in a line parallel to the 84th and 59th Leonis. The two

V. neareſt very unequal. Diſtance 54″ 37‴. The fartheſt extremely unequal.

69. FL. 7 Leonis minoris. In extremo anteriore pede.

Mar. 12, 1782. Double. The largeſt of two. Extremely unequal. L. pr.; S. r. Diſtance 58″ 15‴.

70. FL. 2am Bootis præcedens ad boream.

April 5, 1782. Double. Near 3 degrees n. preceding the 2d Bootis, towards the 43d Comæ Ber.; the preceding of three in a line parallel to α and η Bootis. A little unequal. L. r.; S. darker r. Diſtance 56″ 56‴. Poſition 7° 0′ ſ. preceding.

71. Prope γ (FL. 24am) Geminorum.

April 15, 1782. Double. Three or four minutes n. preceding γ Geminorum. Of the Vth Claſs. More in view.

72. † *m* Herculis. FL. 36 et 37. In ſiniſtro Serpentarii brachio.

May 18, 1782. Double. A little unequal. L. bluiſh w. S. reddiſh w. Diſtance 59″ 59‴. Poſition 36° 57′ ſ. preceding *.

73. τ Urſæ majoris. FL. 14. Duarum in collo præcedens.

June 11, 1782. Double. Extremely unequal. L. w.; S. d. Diſtance 54″ 46‴. Poſition about 45° n. following.

74. S (FL. 72â) Serpentarii borealior.

June 16, 1782. Double. More than 1 degree n. following the 56th double ſtar of the IIId Claſs; nearly in a line parallel to the 62d and 72d Serpentarii, Very unequal. L. rw.; S. r. Diſtance 40″ 54‴. Poſition 39° 15′; inaccurate.

* One of theſe ſtars, at leaſt, ſeems to have changed its place ſince the time of FLAMSTEED, who makes their difference in R.A. 45″, and in P.D. 1′ 35″, Poſition ſ. preceding; hence we have the hypotenuſe or diſtance above 1′ 45″, inſtead of 59″ 59‴, and poſition 69° 46′ inſtead of 36° 57′.

V. 75. E teleſcopicis ε Coronæ borealis ſequentibus.

July 18, 1782. Double. About 1 degree ſ. following ε, in a line parallel to θ and ε Coronæ; the preceding of three forming an arch. Extremely unequal. L. r.; S. darker r. Diſtance 41″ 12‴. Poſition 16° 0′ ſ. following.

76. β Aquarii. Fl. 22. In ſiniſtro humero.

July 20, 1782. Double. Exceſſively unequal. L. w.; S. d. Diſtance about 33″ 16‴; very inaccurate. Poſition 55° 48′.

77. *d* (Fl. 43ª) Sagittarii borealior et ſequens.

Aug. 4, 1782. Double. A few minutes n. following the 43d, in a line parallel to *o* and π Sagittarii; the neareſt of two. Extremely unequal. L. w.; S. d. Diſtance with 278, 36″ 3‴. Poſition 78° 45′ ſ. following.

78. ζ Sagittarii. Fl. 38. Trium ſuper coſtis ſub axilla.

Aug. 4, 1782. Double. Extremely unequal. L. r.; S. d. Diſtance Vth Claſs. Poſition 28° 6′ n. preceding. A third ſtar. Diſtance about four times as far as the former. Poſition alſo n. preceding.

79. Fl. 9 :: Caſſiopeiæ.

Aug. 25, 1782. Double. Of two in a line parallel to β and γ, that towards γ Caſſiopeiæ. Very unequal. L. w.; S. pr. Diſtance 52″ 39‴. Poſition 50° 36′ n. preceding.

80. τ Aquarii. Fl. 69. Duarum in dextra tibia borealior.

Aug. 28, 1782. Double. Very unequal. L. rw.; S. d. Diſtance 36″ 47‴. Poſition 19° 54′ ſ. following.

81. Fl. 35 :: Caſſiopeiæ. In ſiniſtro crure.

Aug. 28, 1782. Double. Conſiderably unequal. L. rw.; S. br. Diſtance 42″ 35‴. Poſition 85° 12′ n. following.

V. 82. *v* (FL. 25am) Caſſiopeiæ præcedens. In ſiniſtra manu.
Aug. 28, Double. Near ¼ degree n. preceding *v*, in a line pa-
1782. rallel to *α* and *β* Caſſiopeiæ. Nearly equal. Both pr.
Diſtance 43″ 26‴. Poſition 7° 48′ n. following.

83. *ψ* Caſſiopeiæ. FL. 36. Sub pede ſiniſtro.
Aug. 28, Double. Very unequal. L. pr.; S. r. Diſtance
1782. 33″ 25‴. Poſition 10° 12′ f. following.

84. FL. 47 :: Caſſiopeiæ. Ex obſcurioribus infra pedes.
Aug. 29, Double. The largeſt of three forming a rectangular
1782. triangle on, or near, the place of the 47th Caſſiopeiæ. A
little unequal. L. rw.; S. pr. Diſtance 50″ 58‴.
Poſition 3° 33′ n. preceding.

85. *ρ* (FL. 27â) borealior et præcedens. In dextro brachio.
Aug. 29, Double. About ⅓ degree n. preceding *ρ* Andromedæ
1782. *θ* verſus. Very unequal. L. rw.; S. r. Diſtance 30″
57‴. Poſition 79° 24′ n. following.

86. FL. 12 Urſæ minoris.
Sept. 4, Treble. Extremely unequal. All three r. The
1782. neareſt is the ſmalleſt. Poſition ſome degrees f. follow-
ing. The fartheſt alſo ſouth, but more following.

87. *σ* Capricorni. FL. 7. Sub oculo dextro.
Sept. 5, Double. Very, or almoſt extremely unequal. L. r.;
1782. S. d. bluiſh. Diſtance 50″ 7‴. Poſition 85° 12′ f.
following.

88. *λ* (FL. 15â) Aurigæ borealior. In ſiniſtra manu.
Sept. 5, Double. About 3′ or 4′ n. following the 15th Au-
1782. rigæ. Very unequal. Diſtance 34″ 15‴, mean mea-
ſure. Poſition 54° 6′ f. preceding.

V. 89. θ Aurigæ. Fl. 37. In dextro carpo.

Sept. 5, 1782. Double. Exceſſively unequal. L. fine w.; S reddiſh. Diſtance with 460, 35″ 18‴, narrow meaſure. Poſition 16° 0′ n. preceding. A third ſtar in view.

90. ν Aurigæ. Fl. 32. In dextri brachii ancone.

Sept. 5, 1782. Double. Exceſſively unequal. L. orange w.; S. r. Diſtance 53″ 43‴. Poſition 61° 48′ f. preceding. S. not viſible till after ſome minutes attention.

91. β (Fl. 34ᵉ) Aurigæ adjecta. In dextro humero.

Sept. 5, 1782. Double. Near ½ degree f. following β, in a line from the 27th continued through β Aurigæ; a conſiderable ſtar. Very or extremely unequal. L. pr.; S. d. Diſtance 30″ 3‴. Poſition 45° 6′ n. preceding.

92. Fl. 3ᵃ Arietis borealior.

Sept. 10, 1782. Double. Full ½ degree f. following the 3d Arietis, in a line parallel to α Arietis and δ Ceti; the moſt ſouth of two. Equal. Both reddiſh. Diſtance 51″ 16‴. Poſition 52° 45′ n. preceding or f. following.

93. Fl. 103ᵃᵐ Herculis ſequens ad auſtrum.

Sept. 19, 1782. Double. About 1¼ degree f. following the 103d Herculis, in a line parallel to the 1ſt and 10th Lyræ; the neareſt of two. Equal, perhaps the following the ſmalleſt. Both r. Diſtance 47″ 46‴. Poſition 45° 42′ f. following.

94. Duarum Fl. 31ᵃᵐ Cephei ſequentium auſtrina.

Sept. 30, 1782. Double. About ¾ degree n. of the 31ſt Cephei, towards α Polaris. Pretty unequal. Both pr. Diſtance 41″ 40‴. Poſition 45° 15′ f. following.

95. Fl. 51 Aquarii. In dextro cubito.

Oct. 2, 1782. Double. Exceſſively unequal. L. rw.; S. d. Diſtance Vth Claſs. Poſition n. preceding. Two other

V. other ſtars in view; the neareſt of them extremely unequal. Poſition about 80 or 90° ſ. preceding. The fartheſt very unequal. Poſition about 30° ſ. following.

96. *υ* (FL. 59^{am}) Aquarii ſequens ad auſtrum.

Oct. 2, 1782. Double. About ½ degree ſ. following *υ*, in a line parallel to *δ* and *c* Aquarii. Extremely unequal. Diſtance Vth Claſs near. Poſition 15 or 20° ſ. preceding.

97. FL. 10 Lacertæ.

Oct. 4, 1782. Double. Very unequal. L. w.; S. r. Diſtance with 278, 52″ 34‴. Poſition 38° 45′ n. following.

98. FL. 3 Pegaſi.

Oct. 4, 1782. Double. Pretty unequal. L. wr.; S. dr. Diſtance 34″ 43‴. Poſition 82° 48′ n. preceding. Beſides II. 62. another ſtar in view. Poſition following.

99. FL. 33 Pegaſi.

Oct. 4, 1782. Double. Conſiderably unequal. L. pr.; S. r. Diſtance with 278, 45″ 3‴. Poſition 89° 12′ n. following.

100. FL. 59 Orionis.

Oct. 4, 1782. Double. The following of two. Extremely unequal. L. w.; S. a point requiring ſome attention to be ſeen. Diſtance 37″ 15‴. Poſition about 65° ſ. preceding.

101. *υ* (FL. 36^{am}) Orionis præcedens.

Oct. 4, 1782. Double. About ⅔ degree preceding *υ*, nearly in a line parallel to *ϰ* and *β* Orionis; the ſecond from *υ*. Extremely unequal. L. w.; S. r. Diſtance 44″ 15‴. Poſition about 15° ſ. following.

V. 102. Fl. 61 Ceti.

Oct. 12, 1782. Double. Extremely unequal. L. rw.; S. dr. Diſtance with 278, 37″ 53‴. Poſition 76° 21′ ſ. preceding. A third ſtar at ſome diſtance. A little unequal. Poſition n. following.

103. Ab ι (Fl. 18ª) Lyræ β verſus.

Oct. 24, 1782. Double. Full ½ degree ſ. preceding ι, nearly towards β Lyræ. Extremely unequal. L. w.; S. r. Diſtance with 278, 45″ 32‴. Poſition 29° 12′ n. following.

104. ε (Fl. 4ª) Sagittæ auſtralior et præcedens.

Nov. 6, 1782. Double. Full ½ degree ſ. preceding ε, in a line parallel to γ Sagittæ and γ Aquilæ; the neareſt of two. Extremely unequal. L. pr.; S. d. Diſtance Vth Claſs. Poſition 16° 18′ ſ. following.

105. γ (Fl. 14ª) Sagittæ auſtralior et ſequens.

Nov. 6, 1782. Double. About ⅓ degree ſ. following γ Sagittæ, in a line parallel to Sagitta and Delphinus. Conſiderably unequal. L. pr.; S. r. Diſtance 38″ 36‴. Poſition 74° 15′ ſ. following.

106. γ (Fl. 12ª) Sagittæ borealior et præcedens.

Nov. 6, 1782. Double. About 1¾ degree n. preceding γ Sagittæ, towards the 6th Vulpeculæ; a conſiderable ſtar. Equal. Both rw. Diſtance 38″ 54‴. Poſition 60° 42′ n. preceding or ſ. following.

107. Fl. 56 Aurigæ.

Nov. 6, 1782. Double. Conſiderably unequal. L. w.; S. pr. Diſtance 52″ 57‴. Poſition 72° 36′ n. following.

108. ϰ (Fl. 13ª) Canis majoris borealior.

Nov. 6, 1782. Double. About ¾ degree n. of ϰ Canis majoris. A little unequal. L. dw.; S. d. Diſtance 42″ 53‴. Poſition 23° 18′ n. following.

V. 109. Inter β Cancri et δ Hydræ.

Nov. 6, 1782. Double. A large ſtar not in FLAMSTEED, between β Cancri and δ Hydræ. Exceſſively unequal. Diſtance 35″ 24‴. Poſition 55° 0′ n. preceding.

110. FL. 111 Tauri.

Nov. 13, 1782. Double. Very unequal. L. rw.; S. r. Diſtance 46″ 42‴. Poſition 3° 48′ n. preceding.

111. FL. 42ª Urſæ majoris auſtralior et ſequens.

Nov. 20, 1782. Double. Full 1 degree ſ. following the 42d, in a line parallel to the 29th and 48th Urſæ majoris; the middle of three forming an arch. Conſiderably unequal. L. wr.; S. r. Diſtance 30″ 40‴. Poſition 51° 27′ n. following.

112. * Ex obſcurioribus μ and ν Geminorum ſequentibus.

Dec. 1, 1782. Double. Forms almoſt an iſoſceles triangle with μ and ν Geminorum. Nearly equal. The preceding pr. the following wr. Diſtance Vth Claſs far.

113. * FL. 9ªᵐ inter et 11ªᵐ Orionis.

Dec. 7, 1782. Treble. About 1½ degree ſ. preceding the 11th Orionis, towards ι Tauri. The two largeſt conſiderably unequal. L. w.; S. pr. Diſtance 37″ 51‴. Poſition 33° 54′ n. preceding. The third farther off and ſmaller. S. r. Poſition n. following.

114. FL. 103 Tauri.

Dec. 7, 1782. Double. Exceſſively unequal. L. rw.; S. d. Diſtance with 278 and 625, 30″ 2‴, mean meaſure. Poſition 72° 24′.

115. *o* Tauri. FL. 114.

Dec. 7, 1782. Double. Exceſſively unequal. L. w.; S. a point. Diſtance 5.″ 34‴. Poſition 77° 54′ ſ. preceding.

V. Two other ſmall ſtars following, and a third to the north.

116. Fl. 41 Arietis.

Dec. 23, 1782. Treble. The two neareſt exceſſively unequal. L. w.; S. a point. Diſtance with 278, 39″ 20‴. Poſition 80° 48′ ſ. preceding. For the diſtance of the fartheſt, ſee VI. 5. *.

117. ζ (Fl. 58am) Arietis præcedens ad boream.

Dec. 23, 1782. Double. About 1¾ n. preceding ζ, towards the 41ſt Arietis; the following of four forming an arch. Very unequal. Both dr. Diſtance 34″ 48‴. Poſition 47° 33′ n. preceding.

118. ε (Fl. 46â) Orionis borealior et præcedens.

Dec. 28, 1782. Double. The moſt n. of three preceding ε Orionis, towards μ Tauri. More north is another ſet of three; care muſt be taken not to miſtake one of them for this. Extremely unequal. L. rw.; S. d. Diſtance Vth Claſs. Poſition 13° 6′ ſ. preceding. Two more following, exceſſively unequal; one about 1′, the other about 1½ minute.

119. ε (Fl. 46â) Orionis auſtralior et præcedens.

Dec. 28, 1782. Double. Full ¾ degree ſ. preceding ε, in a line parallel to ε Orionis, and *b* Eridani; the ſmalleſt and moſt ſ. of two. Very unequal. L. w.; S. r. Diſtance 30″ 12‴; a little inaccurate. Poſition 21° 33 ſ preceding. A third ſtar 2 or 3° ſ. following.

* The ſtar VI. 5. in the place referred to is called Flamsteed's 35th Arietis. With ſo many ſtars and meaſures it was hardly poſſible to avoid ſeveral errors, I have therefore now added to the errata already given at the end of vol. LXXII. and LXXIII. of the Phil. Tranſ. ſome others, that have ſince been detected by a careful review of the double ſtars, and believe that no more will be found.

V. 120. FL. 15 Hydræ.

Dec. 28, 1782. Double. Extremely unequal. L. w.; S. r. Diſtance 43″ 2‴. Poſition about 70° n. preceding.

121. *e* Comæ Berenices. FL. 12.

Jan. 1, 1783. Double. Conſiderably unequal. L. rw.; S. pr. Diſtance 58″ 55‴. Poſition about 77° ſ. following.

122. FL. 44ª Bootis auſtralior et præcedens.

Jan. 8, 1783. Double. Near ⅔ degree ſ. preceding the 44th, towards the 38th Bootis. Very unequal. L. bw.; S. pr. Diſtance 34″ 21‴. Poſition 67° 6′ ſ. preceding.

123. * In Andromedæ pectore.

Jan. 8, 1783. Double. Equal. Both rw. or pr. Diſtance 45″ 1‴. Poſition 32° 24′ ſ. preceding. Its place, as determined in 1777 by C. MAYER, is Æ 0ʰ 34′ 33″ in time, and 29° 45′ 3″ declination north.

124. *g* (FL. 2ªᵐ) Centauri ſequens ad auſtrum.

Jan. 31, 1783. Double. About 1½ degree ſ. following *g* Centauri, in a line parallel to *γ* Serpentis and *θ* Centauri; the moſt ſ. of two. Conſiderably unequal. Diſtance 54″ 1‴; too low for accuracy.

125. FL. 46ªᵐ Bootis ſequens ad boream.

Feb. 3, 1783. Double. Near 2 degrees n. following the 46th, in a line parallel to *ζ* Bootis and *β* Coronæ; the third ſtar about that direction. Conſiderably unequal. L. r.; S. darker r. Diſtance 33″ 53′. Poſition 37° 33′ ſ. preceding.

126. *r* (FL. 5ªᵐ) Herculis præcedens ad auſtrum.

Feb. 3, 1783. Double. Near ½ degree ſ. preceding *r* Herculis, in a line parallel to *γ* and *δ* Serpentis; a ſmall ſtar. A little unequal. Both pr. Diſtance 37″ 51‴, rather full meaſure. Poſition 52° 6′ ſ. preceding.

V. 127. (Fl. 41am) Herculis præcedens ad boream.

Feb. 5, 1783. Double. About ¾ degree n. preceding the 41ſt Herculis, in a line parallel to x Serpentarii and β Herculis. Pretty unequal. Both r. Diſtance 48″ 40‴. Poſition 19° 45′ n. preceding.

128. ι (Fl. 68am) Virginis ſequens.

Feb. 7, 1783. Double. About 1½ degree following ι Virginis, in a line parallel to Spica and β Libræ. A little unequal. L. pr.; S. r. Diſtance 41″ 58‴.

129. *f* (Fl. 25am) Virginis ſequens ad boream.

Feb. 7, 1783. Double. About 1½ degree n. following *f*, in a line parallel to γ and ε Virginis; a large ſtar. Very unequal. L. r.; S. dark r. Diſtance 46″ 42‴. Poſition 6 or 7° ſ. following. A double ſtar of the Vth Claſs in view, preceding.

130. Fl. 35 Comæ Berenices.

Feb. 26, 1783. Double. Very unequal. L. r.; S. d. Diſtance 31″ 17‴. Poſition 36° 51′ ſ. following.

131. Fl. 24am Libræ ſequens ad boream.

Mar. 1, 1783. Double. About 1½ degree n. following the 24th Libræ, in a line parallel to π and β Scorpii. Conſiderably unequal. L. rw.; S. r. Diſtance 47″ 46‴.

132. Fl. 29am inter et 30am Libræ.

Mar. 1, 1783. Double. Of two between the 29th and 30th Libræ that neareſt to the 30th. Very unequal. L. w.; S. d. Diſtance 39″ 59‴; very inaccurate.

133. Fl. 60 Herculis.

Mar. 7, 1783. Double. Extremely unequal. L. w.; S. d. Diſtance 48″ 40‴. Poſition 37° 0′ n. preceding.

V. 134. ψ (FL. 4^{am}) Ophiuchi præcedens ad auſtrum.

Mar. 24, 1783. Double. About 1 degree preceding and a little ſ. of ψ, in a line parallel to ψ Ophiuchi and ω Scorpii; the fartheſt of two in the baſe of a triangle. Equal. Diſtance 45″ 47‴.

135. Ad FL. 49^{am} Camelopardali.

April 4, 1783. Double. The ſmalleſt and moſt ſ. of two that are about 20′ aſunder. A little unequal. Both r. Diſtance with 278, 38″ 18‴. Poſition 85° 0′ ſ. preceding.

136. θ (FL. 65^{a}) Aquilæ borealior.

Sept. 12, 1783. Double. About $\frac{2}{3}$ degree n. of θ, in a line parallel to η and β Aquilæ; a conſiderable ſtar. Conſiderably unequal. L. pr.; S. r.; Diſtance with 278, 47″ 5‴. Poſition 65° 48′ ſ. preceding.

137. χ (FL. 17^{a}) Cygni borealior.

Sept. 22, 1783. Double. About 1 $\frac{1}{3}$ degree n. of χ, towards δ Cygni; a conſiderable ſtar. Conſiderably unequal. L. garnet; S. r. Diſtance with 278, 35″ 1‴. Poſition 57° 3′ n. following.

SIXTH CLASS OF DOUBLE STARS.

VI. 67. η Orionis. FL. 28. In extremo enſis manubrio.

Dec. 27, 1781. Double. Exceſſively unequal. L. w.; S. d. Diſtance 1′ 50″ 57‴. Poſition 35° 12′ n. following.

VI. 68. η (Fl. 28[a]) Orionis auſtralior.

Dec. 27, 1781. Double. About $\frac{1}{4}$ degree ſ. of, and a little following η, in a line nearly parallel to δ and θ Orionis. Very unequal L. r.; S. d. Diſtance 2′ 0″ 11‴. Poſition 7° 54′ n. preceding.

69. Fl. 14 Arietis. Supra caput.

Dec. 27, 1781. Double. Very unequal. L. pr.; S dr. Diſtance 1′ 29″ 28‴. Poſition 11° 12 n. preceding.

70. *o* Geminorum. Fl. 70. Supra caput prioris II[i].

Dec. 27, 1781. Treble. Or two ſmall ſtars in view; the neareſt a little more than 1 minute; the other not much farther.

71. τ Hydræ. Fl. 31. Trium in flexu colli auſtraliſſima.

Jan. 20, 1782. Double. Pretty unequal. L. w. inclining to roſe colour. S. pr. Diſtance 1′ 1″ 40‴. Poſition 88° 36′ n. preceding.

72. Ad Fl. 68[am] Orionis. In fuſte.

Jan. 30, 1782. Double. The moſt n. of two that are 1 degree aſunder. Very unequal. L. w.; S. dr. Diſtance with 278, 1′ 12″ 50‴. Poſition 41° 0′ ſ. preceding.

73. ϵ Geminorum. Fl. 27. In boreali genu præcedentis II[i].

Feb. 2, 1782. Double. L. w. Diſtance 1′ 50″ 30‴.

74. Fl. 51 Geminorum.

Feb. 2, 1782. Has two very obſcure ſtars in view. L. r.; S. r. S. r. The neareſt about 1½, the next 2 minutes. Poſition of both about 40 or 50° n. following.

75. ω Cancri. Fl. 4. Ad primum borealem forficem.

Feb. 2, 1782. Has a very obſcure ſtar in view. L. pr. Diſtance about 1¼ minute. Poſition about 30° n. preceding. A third about 2′. Poſition more north.

VI. 76. *o* Leonis. FL. 14.

Feb. 2, 1782. Double. Extremely unequal. L. rw.; S. r. Distance 1′ 3″ 29‴. Position 49° 36′ n. following.

77. *x* Virginis. FL. 93.

Feb. 4, 1782. Double. Very unequal. L. w.; S. dr. Distance 1′ 8″ 22‴.

78. ζ (FL. 16am) Cancri sequitur.

Feb. 8, 1782. Double. About ½ degree following ζ Cancri, towards η Leonis. Extremely unequal. Distance 1′ 3″ 47‴.

79. φ Leonis. FL. 74,

Feb. 9, 1782. Double. Very unequal. L. w.; S. pr. Distance 1′ 38″ 35‴. Position about 10 or 12° n. preceding.

80. FL. 93 Leonis.

Feb. 9, 1782. Double. Very unequal. L. w.; S. db. Distance 1′ 10″ 13″.

81. FL. 27 Virginis. In ala dextra.

Feb. 9, 1782. Double. Extremely unequal. L. w. Distance 1′ 28″ 48‴.

82. FL. 31 Monocerotis. In media cauda.

Feb. 9, 1782. Double. Very unequal. L. rw.; S. db. Distance 1′ 10″ 13‴. Position 40° 0′ n. preceding.

83. Prope FL. 1am Orionis.

Feb. 9, 1782. Double. A few minutes s. following the 1st, towards the belt of Orion. Considerably unequal. L. pr.; S. r. Distance 1′ 20″ 58‴. Position 88° 15′ n. following.

84. FL. 14 Canis minoris.

Feb. 9, 1782. Treble. The nearest extremely unequal. L. rw.; S. d. Distance 1′ 5″ 28‴. Position 26° 24′ n. following.

VI. lowing. The third forms an angle, a little larger than a rectangle, with the other two. Poſition ſ. following.

85. Fl. 27 Hydræ.

Feb. 9. 1782. Double. Very unequal. L. rw.; S. pr. Diſtance VIth Claſs far. Poſition about 60° ſ. preceding.

86. Prima ad σ Cancri. Fl. 51.

March 5, 1782. Double. Extremely unequal. L. w.; S. d. Poſition n. following.

87. Tertia ad σ Cancri. Fl. 64.

March 5, 1782. Double. Very unequal. L. rw.; S. dr. Diſtance 1′ 25″ 45‴. Poſition 25° 12′ n. preceding.

88. β Aurigæ. Fl. 34. In dextro humero.

March 5, 1782. Double. Extremely or exceſſively unequal. L. fine bluiſh w.; S. d. Diſtance 2′ 49″ 6‴. Poſition 54° 12′ n. following. A third farther off. Very unequal. About 40 or 50° n. following.

89. Fl. 6ᵉ Bootis adjecta.

Mar. 12, 1782. Double. Juſt following the 6th Bootis. A little unequal. L. r.; S. deeper r. Diſtance 1′ 19″ 39‴. Poſition 58° 6′ ſ. preceding.

90. Fl. 61 Virginis.

Apr. 3, 1782. Double. Very unequal. L. w.; S. d. Diſtance 1′ 13″ 15‴. Poſition about 75° n. preceding.

91. Prope γ (Fl. 24ᵃᵐ) Geminorum.

Apr. 15, 1782. Double. Three or four minutes n. of γ Geminorum. Conſiderably unequal. Both ſmall; too obſcure for meaſures with 7-feet; my 20-feet ſhews a third ſtar between them with 12 inches aperture.

VI. 92. ξ (FL. 1ª) Capricorni borealior.

June 14, 1782. Double. About ⅓ degree n. of ξ Capricorni. Very unequal. Both r. Diſtance 1′ 2″ 16‴. Poſition 2° 3′ f. preceding.

93. ϱ Coronæ borealis. FL. 15. Ad ſummum.

July 18, 1782. Double. Very unequal. L. w.; S. d. Diſtance 1′ 27″ 44‴; a little inaccurate. Poſition 54° 27′ f. following.

94. λ Coronæ borealis. FL. 12.

July 18, 1782. Double. Extremely unequal. L. w.; S. r. Diſtance 1′ 35″ 14‴. Poſition 33° 12′ n. following.

95. η Bootis. FL. 8. Trium in ſiniſtro crure borea.

Aug. 3, 1782. Double. Extremely unequal. L. w. inclining to orange; S. r. Diſtance about 1½ minute. Poſition about 25 or 30° f. following.

96. ζ Perſei. FL. 44. In pede ſiniſtro.

Aug. 25, 1782. Treble. The neareſt extremely unequal. L. w.; S. r. Diſtance 1′ 11″ 26‴. Poſition 66° 36′ f. preceding. The fartheſt very unequal. S. r. about 1½ minute. 70 or 75° f. preceding.

97. Secunda ad τ Aquarii. FL. 71. In dextro crure.

Aug. 28, 1782. Double. Very unequal. L. r.; S. d. Diſtance 2′ 3″ 36‴, mean meaſure. Poſition 18° 30′ n. preceding.

98. FL. 46ᵃᵐ Tauri ſequens ad auſtrum.

Sept. 7, 1782. Double. About 1½ degree f. following the 46th, nearly in a line parallel to the 38th Tauri and the 42d Eridani. A little unequal. L. pr.; S. r. Diſtance 1′ 2″ 34‴. Poſition 43° 48′ n. preceding. A double ſtar of the Vth Claſs in view, following within 3′.

Equal.

VI. Equal. Both ſmall and r. Almoſt ſimilarly ſituated with the above, but poſition more n. preceding.

99. *m* Perſei. Fl. 57. In dextri pedis talo.

Sept. 7, 1782. Double. Pretty unequal. L. r.; S. rw. Diſtance 1′ 36″ 27‴. Poſition 71° 51′ ſ. preceding.

100. *ι* (Fl. 32am) Cephei ſequens.

Sept. 30, 1782. Double. About 1¾ degree n. following *ι*, nearly towards *γ* Cephei. A little unequal. Both pr. Diſtance 1′ 1″ 54‴. Poſition 8° 9′ n. preceding.

101. *δ* Tauri. Fl. 68.

Oct. 31, 1782. Has two ſtars in view. The neareſt exceſſively unequal. L. w.; S. d. Diſtance with 2, 8, 1′ 3″ 18‴. Poſition 35° 24′ ſ. preceding. The fartheſt extremely unequal. S. r. About 1½ minute. Poſition about 50h n. preceding.

102. Fl. 5 Lyncis.

Nov. 13, 1782. Double. The largeſt of a ſmall triangle. Very unequal. L. r.; S. garnet. Diſtance 1′ 28″ 20‴. Poſition 2° 0′ n. preceding.

103. *ε* Pegaſi. Fl. 8.

Nov. 20, 1782. Double. Very unequal. L. pr.; S. dr. Diſtance 1′ 30″ 56‴. Poſition 52° 45′ n. preceding.

104. *ζ* Bootis. Fl. 30. In dextro calcaneo.

Nov. 29, 1782. Has a very obſcure ſtar in view. Extremely unequal. L. w. inclining to r.; S. d. Diſtance about 1½ minute. Poſition almoſt directly preceding.

105. Fl. 105 Tauri.

Dec. 7, 1782. Double. Very unequal. L. pr.; S. r. Diſtance 1′ 41″ 29‴. Poſition 18° 0′ ſ. preceding.

VI. 106. *b* Eridani. FL. 62.

Dec. 7, 1782. Double. Conſiderably unequal. L. w.; S. pr. Diſtance 1′ 0″ 26‴. Poſition 15° 9′ n. following.

107. FL. 31ª Monocerotis auſtralior et præcedens.

Dec. 21, 1782. Double. About 1¼ degree ſ. of, and a little preceding the 31ſt Monocerotis, in a line parallel to ζ Hydræ and the 31ſt Monocerotis; the moſt ſouth of two. Conſiderably unequal. L. r.; S. deeper r. Diſtance about 1½ minute. Poſition 50 or 60° ſ. following.

108. θ (FL. 22ª) Hydræ borealior et præcedens.

Dec. 28, 1782. Double. About ½ degree n. of, and a little preceding θ, nearly in a line parallel to α and θ Hydræ. Very unequal. L. r.; S. blackiſh r. VIth Claſs far. Poſition 1 or 2° n. preceding. A third ſtar preceding.

109. FL. 22 an 26 Cancri incertum.

Dec. 29, 1782. Double. One of the two being loſt *, it does not appear which is the remaining ſtar. Very unequal. L. r.; S. dr.

110. Teleſcopica ad *o* Ceti.

Jan. 2, 1783. Double. Looking for *o* Ceti, which was inviſible to the naked eye, I miſtook this for it. Pretty unequal. L. rw. of about the eighth magnitude; S. r. Diſtance 1′ 20″ 52‴. Poſition 33° 42′.

111. α Hydræ. FL. 30. Duarum contiguarum lucidior.

Jan. 8, 1783. Has two ſtars within about 2 minutes; the neareſt exceſſively unequal; the fartheſt extremely unequal. Both ſ. following.

112. FL. 13 Bootis.

Jan. 8, 1783. Double. Extremely unequal. L. r.; S. dr. Diſtance 1′ 17″ 58‴. Poſition 7° 24′ n. preceding.

* See Phil. Tranſ. vol. LXXIII. p. 252.

VI. 113. Fl. 4 Virginis.

Jan. 8. Double. Extremely unequal. L. wr.; S. dr. Dif-
1783. tance 2′ 25″ 44‴; too obſcure for accuracy.

114. Fl. 69am Orionis præcedens ad auſtrum.

Jan. 9, Double. About ½ degree ſ. preceding the 69th,
1783. nearly towards λ Orionis. Conſiderably unequal. L. pr.; S. d. Diſtance 1′ 30″ 38‴. Poſition 22° 6′ ſ. following.

115. Fl. 21am Crateris ſequens ad auſtrum.

Jan. 10, Double. About 2½ degree ſ. following the 21ſt, in
1783. a line parallel to the 12th Crateris and 4th Corvi. Very unequal. L. w.; S. r. Poſition 12° 12′ n. following.

116. Fl. 43 Herculis.

Jan. 10, Double. Very unequal. L. inclining to garnet;
1783. S. r. Diſtance 1′ 14″ 37‴. Poſition 38° 48′ ſ. preceding.

117. Fl. 12â Libræ borealior et præcedens.

Jan. 10, Double. About 1¼ degree n. preceding the 12th
1783. Libræ, towards Spica. Very unequal. L. rw.; S. r. Poſition about 40° ſ. preceding.

118. Fl. 30 Monocerotis.

Feb. 12, Double. Very or extremely unequal. Diſtance
1783. 3′ 30″ 54‴ *.

119. ε (Fl. 18â) Piſcis auſtrini auſtralior et præcedens.

July 28, Double. About 1¼ degree ſ. of, and a little pre-
1783. ceding ε Piſcis auſtrini, in a line from δ Aquarii continued

* On account of the change in the magnitudes of the 1ſt and 2d Hydræ, this ſmall ſtar may be of uſe to aſcertain whether the 30th Monocerotis, which is ſituated between them, has any conſiderable proper motion. See Phil. Tranſ. vol. LXXIII. p. 255.

through

through ε Piſcis. Pretty unequal. L. dpr. S. dr. Diſtance 1′ 26″ 58‴. Poſition 67° 46′ ſ. following.

120. FL. 43ᵐ Sagittarii ſequens ad auſtrum.

Aug. 16, 1783. Double. Near 1 degree ſ. following the 43d, in a line parallel to ξ and *o* Sagittarii; a conſiderable ſtar. Very unequal. Both dr. Diſtance with 278, 1′ 14″ 9‴. Poſition 37° 0′ n. preceding.

121. FL. 12 Lacertæ.

Aug. 18, 1783. Double. Very unequal. L. w.; S. r. Diſtance with 278, 1′ 0″ 10‴. Poſition 73° 0′ n. following.

Add the following errata of the Catalogue of Double Stars in vol. LXXII. to thoſe already noticed at the end of the LXXIId and LXXIIId volumes.

Page.	Line.	For	Read
133	22	25.	25*.
140	3	19″ 14‴	19″ 26‴
145	26	35″ 48‴	36″ 9‴
153	7	π Capricorni. FL. 10.	ϱ Capricorni. FL. 11.
153	11	33° 42′	61° 23′
156	4	FL. 5.	FL. 4.

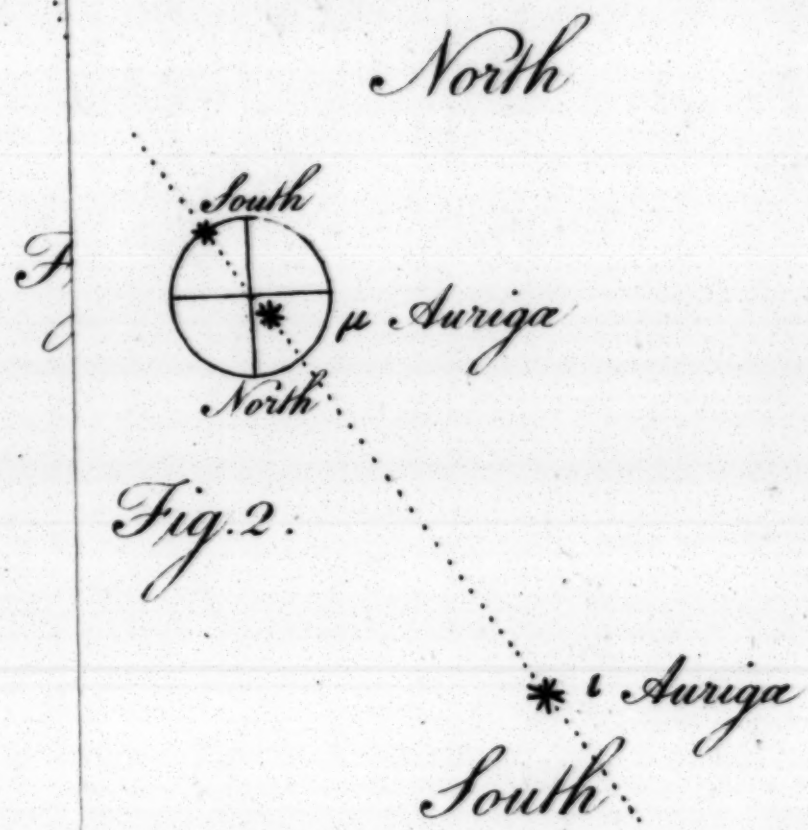


Fig. 2.

Basire Sc.

Fig. 1.

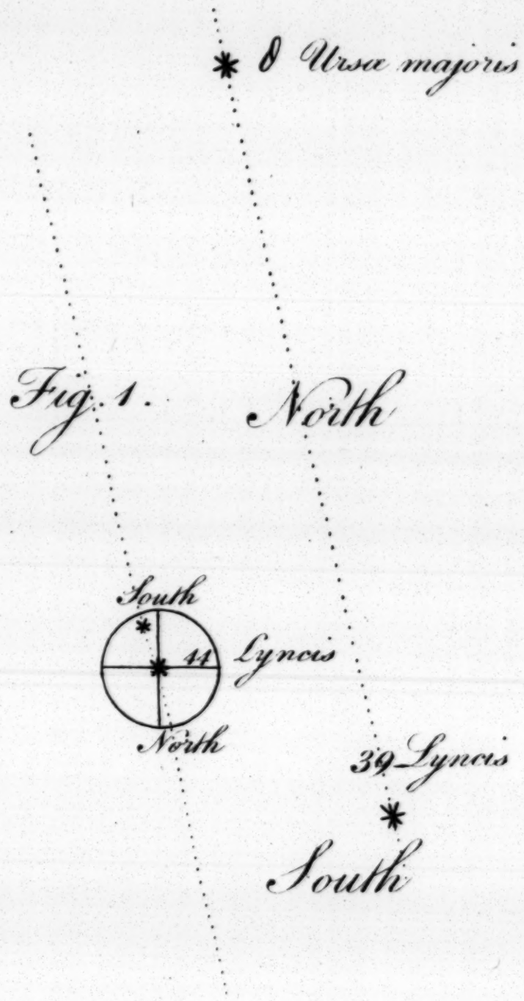
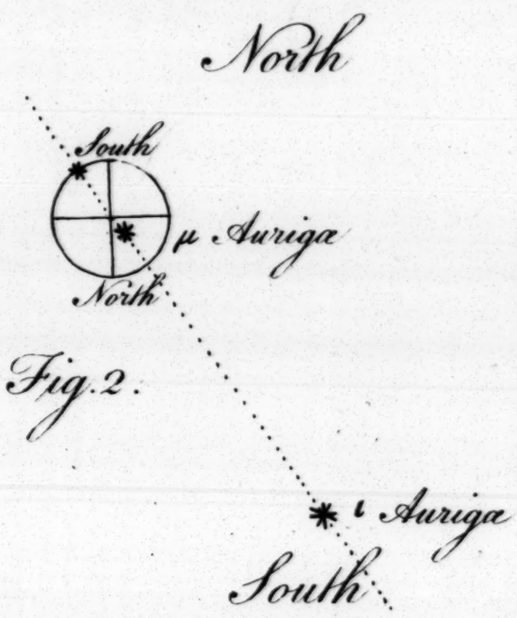

δ Ursæ majoris
North
South
41 Lyncis
North
39 Lyncis
South

Fig. 2.


North
South
μ Auriga
North
ι Auriga
South

VII. *Obſervations of a new variable Star. In a Letter from* Edward Pigott, *Eſq. to Sir* H. C. Englefield, *Bart. F. R. S. and A. S.*

Read December 23, 1784.

DEAR SIR,

FOR ſome years paſt I have been employed in verifying all the ſtars ſuſpected to be variable, in order that hereafter we may know with certainty what to depend upon. This undertaking, which is nearly completed, has already proved of uſe in detecting many miſtakes, and producing ſome diſcoveries; among which, the following is one of the moſt important. September 10, 1784, I firſt perceived a change in the brightneſs of the ſtar η Antinoi, and by a ſeries of obſervations made ever ſince, I find it ſubject to a variation very ſimilar to that of Algol, though not exactly the ſame in any one particular.

η Antinoi, when brighteſt, is of the third or fourth magnitude, being between δ and β Aquilæ; and at its leaſt brightneſs of the fourth or fifth magnitude, it then being between that of ι Antinoi and μ Aquilæ; therefore, its greateſt variation in brightneſs may be called about one magnitude; and the changes it undergoes, though probably not nicely aſcertained from ſo few obſervations, are nearly theſe:

At its greateſt brightneſs	44 ± hours.
In decreaſing - -	62 ± hours.
At its leaſt brightneſs	30 ± hours.
In increaſing - -	36 ± hours.

All theſe changes, which hitherto ſeem to be regular and conſtant, are performed in 7 days 4 hours 38—minutes; this I ſhall ſtile its period, and hereafter will ſhew how it is determined with ſuch exactneſs.

The ſtars to which η Antinoi was compared are in order thus: δ Aquilæ third magnitude, β Aquilæ and θ Serpentis fourth magnitudes, ι Antinoi fourth or fifth magnitude, and μ Aquilæ a bright fifth. I find, by ſeveral years obſervation, that β Aquilæ retains the ſame brightneſs. ι Antinoi, which has been examined with particular attention by Mr. GOODRICKE and myſelf, is ſuſpected by us both to be ſubject to a ſmall variation, but not ſufficiently apparent, ſo as to affect materially theſe compariſons, and poſſibly it may be only the effect of ſome optical illuſion; for I have frequently remarked, that both in the twilight and moon-light, or when the air is in the leaſt hazy, there is a greater difference between the brightneſs of many of the ſtars, than in a dark night and clear ſky.

In the following journal of obſervations of η Antinoi, the Greek letters β, δ, μ, belong to Aquila, and ι, ν, to Antinous; ſecondly, the magnitudes marked in column the third are by eſtimation, and can be of no further uſe than merely to give, at firſt ſight, an idea of the ſtar's brightneſs; and laſtly, the lines diſtinguiſhed by inverted commas, are extracts from Mr. GOODRICKE's journal, whoſe friendly aſſiſtance I have often experienced, and was the more welcome on this occaſion, becauſe repeated attention and great exactneſs were requiſite.

Dates. 1783.	Hours.	Magnitude.	Journal of the comparative brightneſs of η Antinoi.
July 17	10±	3 · 4	Leſs than δ Aquilæ and brighter than θ Serpentis (β Aquilæ and θ Serpentis are equal) weather hazy.
19	10±	4	Rather brighter than β Aquilæ and θ Serpentis
27	10±	4	If any difference, leſs than β Aquilæ.
			N. B. Theſe times are from recollection, and cannot err more than 1½ hour.
1784			
Sept. 10	10±	4	Leſs than β Aquilæ and θ Serpentis.
12	7½	4 · 5	Much leſs than β, equal to ι.
—	9	4 · 5	"A little brighter than ι, air clear."
13	7½ 9½	3 · 4	Leſs than δ, brighter than β, and much brighter than ι.
—	8	3 · 4	"Brighter than ι and β."
15	8	4	Rather brighter than β, and much brighter than ι.
18	9½ 11	4 · 5	"Leſs than β and ι."
19	7½	4 · 5	Much leſs than β, and equal to ι.
—	7 9	4 · 5	"Leſs than β and ι."
20	8	3 · 4	Brighter than β and ι; at 11 h. it ſeemed to have increaſed.
23	7½ 8	3 · 4	Leſs than δ, rather brighter than β; thought it rather leſs at 11½ h.; moon near.
28 29	9±	3 · 4	Brighter than β; moon-light.
30	9±	3 · 4	If any difference, rather brighter than β.
—	9±	3 · 4	"Rather brighter than β."
Oct. 1	7 9	4	Leſs than β, brighter than ι; air clear, moon-light.
2	8	4 · 5	Equal to ι, much leſs than β.
—	8	4 · 5	"Leſs than ι."
5	8	3 · 4	Between the brightneſs of β and δ.
6	9±	3 · 4	"Brighter than β and ι."
7	7½ 9¾	3 · 4	Rather brighter than β.
—	8±	3 · 4	"Much the ſame as yeſterday."
8	8±	4	Brighter than ι; think it not leſs than β; this obſervation doubtful, occaſioned by intervening clouds.
—	8±	4 · 5	"Believe it leſs than ι; weather bad."
9	8±	4 · 5	"Certainly leſs than β; weather bad."
10	11	4 · 5	"Leſs than ι; rather a doubtful obſervation."
11	10½	4	Rather leſs than β, and brighter than ι.
15	10	4	Equal to β.
—	8±	4	"Rather brighter than β."
16	8	4	Leſs than β, brighter than ι.

Dates. 1784	Hours.	Magnitude.	Journal continued.
Oct. 16	$6\frac{1}{2}$	4 · 5	"Leſs than β and ι."
17	$\left\{\begin{matrix}7\\8\end{matrix}\right\}$	4 · 5	Undoubtedly leſs than ι.
18	8	4 · 5	Leſs than ι, brighter than μ.
—	$6\frac{1}{2}$	4 · 5	"Leſs than ι."
19	$7\frac{1}{2}$	3 · 4	Evidently brighter than β.
20	$8\pm$	3 · 4	"Much brighter than β."
22	$8\pm$	3 · 4	"Brighter than β."
23	$6\frac{1}{2}$	4	Leſs than β.
—	8	4	"Not ſo bright as β, brighter than ι."
24	7	4 · 5	Equal to ι, much leſs than β; moon-light, air clear.
—	$\left\{\begin{matrix}6\frac{1}{2}\\7\end{matrix}\right\}$	4 · 5	"Leſs than ι; rather, though very little, brighter than μ."
25	$6\frac{3}{4}$	4 · 5	Much leſs than β, equal to ι, brighter than μ.
26	$6\frac{1}{2}$	4	Sometimes ſeemed rather leſs, but generally equal to β.
—	$9\frac{1}{4}$	4	Equal, if not rather brighter than β.
—		4	"At $6\frac{1}{2}$ rather leſs, at $8\frac{1}{2}$ nearly equal, and at $9\frac{1}{2}$ "rather brighter than β."
27	6	3 · 4	Remarkably bright, nearer δ than β; moon-light, air clear.
—	$6\frac{1}{2}$	3 · 4	"Nearer to β than to δ."
31	$8\frac{1}{4}$	4	Seemed equal to β; air not very clear.
Nov. 3	$5\frac{1}{2}$	3 · 4	"Rather brighter than β."
6	$8\frac{1}{2}$	4	Evidently leſs than β.
7	9	4 · 5	Much leſs than β.
—	$7\frac{1}{2}$	4 · 5	"Leſs than β and ι."
11	7	3 · 4	Brighter than β, much leſs than δ.
12	$6\frac{1}{2}$	3 · 4	Rather brighter than β, certainly equal.
—	$8\frac{1}{2}$	3 · 4	"Rather brighter than β and ι."
13	$\left\{\begin{matrix}5\frac{1}{2}\\7\end{matrix}\right\}$	4 · 5	Leſs than β, equal to ι.
—	7	4 · 5	"Leſs than β, and rather leſs than ι."
16	$5\frac{3}{4}$	4	Evidently leſs than β, and rather brighter than ι; at 8 it ſeemed increaſed, and about
—	8	4	Between its leaſt and full brightneſs.
—	7	4 · 5	"Leſs than β, and ſomething leſs than ε."
17	$\left\{\begin{matrix}5\frac{3}{4}\\7\frac{1}{4}\end{matrix}\right\}$	3 · 4	Brighter than β.
19	6	4	If any difference, rather brighter than β; clouds cov-
—	8	4	ered the moon: at 8h. if any difference rather leſs than β; moon-light, and air not ſo clear as at 6.
—	6	4	"Rather brighter than β, brighter than ι."
20	7	4	Rather leſs than β, brighter than ι.
21	$6\frac{3}{4}$	4	Leſs than β, rather brighter than ι; moon-light.
25	7	3 · 4	"Brighter than β."
Dec. 4	$6\frac{1}{2}$	4	If any difference, leſs than β.

In

In order to obtain a point of compariſon, for ſettling the periodical changes of η Antinoi, which I ſuppoſe to be conſtant, it is natural to fix upon that phaſis, which can be determined with the greateſt preciſion; and this ſeems to be at the time when it is between its leaſt and greateſt brightneſs, as *almoſt the whole* increaſe of brightneſs is completed in leſs than 24 hours, though the perfect completion is performed only in 36 ± hours; thus having ſettled this neceſſary point, and found roughly the length of a ſingle period, the computations, in order to obtain greater exactneſs, are as follows.

Time when η Antinoi was between its leaſt and greateſt brightneſs.	Intervals between the obſervations.	Number of periods in ditto.	Length of a ſingle period.
Hours.	Days. Hours.		Days. Hours.
1784, Sept. 12. at 20 / Oct. 11. at 11	28 15	4 each of	7 $3\frac{3}{4}$
Sept. 12. at 20 / Oct. 18. at 20	36 0	5 D°	7 $4\frac{3}{4}$ +
Sept. 12. at 20 / Oct. 26. at 00	43 4	6 D°	7 $4\frac{3}{4}$ −
Sept. 12. at 20 / Nov. 16. at 8	64 12	9 D°	7 4
Sept. 19. at 20 / Oct. 18. at 20	2 9 0	4 D°	7 6
Sept. 19. at 20 / Oct. 26. at 00	36 4	5 D°	7 $5\frac{1}{2}$ +
Sept. 19. at 20 / Nov. 16. at 8	57 12	8 D°	7 $4\frac{1}{2}$
Oct. 11. at 11 / Nov. 16. at 8	35 21	5 D°	7 $4\frac{1}{4}$ −
Oct. 18. at 20 / Nov. 16. at 8	28 12	4 D°	7 3
Length of a ſingle period, on a mean,			7 4 30

Perhaps other aſtronomers may not exactly agree with me, in fixing the times as ſet down in column the firſt; for my part, I determined them without paying any regard to the reſults, by taking a medium between the times when η Antinoi

had rather paſſed its leaſt brightneſs, being nearly equal to ι Antinoi, and when it was a little, but undoubtedly, brighter than β Aquilæ. Though it does not appear, as I have already ſaid, that any of the other phaſes can be ſettled with equal preciſion, different compariſons nevertheleſs may prove ſatisfactory towards corroborating the above; I have therefore alſo deduced its period from the beſt and moſt diſtant obſervations, made when at its leaſt brightneſs; they are thus: 7 days 0 hours and 7 days 5 hours. Theſe reſults I reject, and retain the mean given by the firſt ſet, with which we may proceed on to gain a much greater exactneſs; let one period be ſubtracted from the obſervation of July 27th, 1783, and it will appear, that η Antinoi had varied in brightneſs during the following four days, though at that time it did not ſtrike me.

1783, July 17th, decidedly brighter than β Aquilæ.
— 18th, not obſerved.
— 19th, rather brighter than β Aquilæ.
— 20th (anſwering to the 27th) equal or rather leſs than β Aquilæ.

As it is therefore evident, that on July 19th and 27th, 1783, η Antinoi was *decreaſing* in brightneſs, I ſhall compare thoſe days obſervations to correſponding ones made in 1784.

		Hours.	
1784,	Sept. 30. at	6	Similar obſervations to that of 1783 July 19th, at 10 h. ±, η Antinoi being rather brighter than β Aquilæ.
	Oct. 7. at	16	
	Oct. 15. at	6	
	Oct. 22. at	12	
	Nov. 12. at	2	
	Nov. 19. at	00	

1784,

	Hours.	
1784, Sept. 30. at	18	
Oct. 15. at	14	Similar Obſervations to that of 1783,
Oct. 22. at	19	July 27th, at 10 h. ±, η Antinoi being
Nov. 12. at	14	equal to or rather leſs than β Aquilæ.
Nov. 19. at	14	

In eſtimating the above times, I paid much attention to the obſervations of the preceding and following days; however, a few hours more or leſs do not make a material difference. The reſults of theſe compariſons are

D.	H.	M.
7	4	39½
7	4	44½
7	4	53½
7	4	54⅔
7	4	32
7	4	26½
7	4	32
7	4	42½
7	4	43 –
7	4	26
7	4	21½
On a mean 7	4	38 – length of a ſingle period.

As this approaches the moſt to the preceding reſult, it may be aſſumed as neareſt the truth, provided the changes be uniformly periodical.

Hitherto the opinion of aſtronomers concerning the changes of Algol's light ſeem to be very unſettled; at leaſt none are univerſally adopted, though various are the hypotheſes to account for it; ſuch, as ſuppoſing the ſtar of ſome other than a ſpherical

a ſpherical form, or a large body revolving round it, or with ſeveral dark ſpots or ſmall bright ones on its ſurface, alſo giving an inclination to its axis, &c.; though moſt of theſe conjectures with regard to Algol be attended with difficulties, ſome of them combined do, I think, account for the variation of η Antinoi.

Thoſe perſons who are accuſtomed to examine the ſtars attentively will not be ſurpriſed to find, that Mr. GOODRICKE and I do not always perfectly agree in our obſervations; theſe ſmall differences in the magnitudes of the ſtars are very difficult to be aſcertained with the naked eye, which has often made me lament, we had not ſome contrivance for determining their relative brightneſs, and even I attempted ſeveral methods, but did not purſue them with ſufficient attention and diligence to obtain any ſatisfactory reſults; nevertheleſs I ſhall juſt mention them, as perhaps ſomebody elſe may overcome thoſe difficulties, which to me appeared ſo very conſiderable.

1. In 1778 I had ſmall pieces of fine glaſs ſtained with different ſhades, which being applied to the eye end of a teleſcope, I could eaſily find what degree of ſhade was requiſite to efface ſtars of different brightneſs; and thus I obſerved ſome of the ſtars and planets.

2. Diaphragms were attempted; but, beſides other difficulties, they did not efface ſtars of the firſt magnitude.

3. A method which pleaſed me much, and perhaps may not prove unſucceſsful, is, by putting the ſtars out of the focus of a teleſcope till they become inviſible; this is performed by drawing the eye-tube of a refractor either in or out; the point of focal diſtance being previouſly determined, the brighter the ſtar the greater length of tube muſt be ſlid either in or out to efface it; thus I was in hopes of determining their magni-

tudes, and for that purpoſe had in 1776 diviſions engraved on the eye-tube of a refractor; but found that its high magnifying powers prevented ſtars of the firſt and ſecond magnitude becoming inviſible.

Laſtly, I am inclined to think the following method practicable, *viz.* to reflect in a teleſcope, by means of an illuminator, different degrees of light in a known proportion, ſo that ſtars of all magnitudes may be obliterated.

The changeable ſtate of the weather will perhaps be thought a conſiderable obſtacle to theſe contrivances, and to throw doubt on the obſervations; but this may be ſufficiently obviated by attending to ſmall teleſcopic ſtars, which according to the clearneſs of the atmoſphere are more or leſs diſtinctly ſeen.

I beg the favour of you, dear Sir, to preſent theſe obſervations to the Royal Society; and believe me, with the greateſt regard, &c.

York, Dec. 5, 1784. EDWARD PIGOTT.

VIII. *Aſtronomical Obſervations. In two Letters from M.* Francis de Zach, *Profeſſor of Mathematics, and Member of the Royal Academies of Sciences at* Marſeilles, Dijon, *and* Lyons, *to Mr.* Tiberius Cavallo, *F. R. S.*

Read December 23, 1784.

SIR, Lyons, April 4, 1783.

I SEND you the account of the obſervations on the eclipſe of the moon, which I have made together with the rev. Father LE FEVRE, Aſtronomer at Lyons, in the Obſervatory called *au grand Collège*; to which I ſhall add the obſervations of the vernal equinox; ſome obſervations on Jupiter's ſatellites, made at Marſeilles by M. SAINT JACQUES DE SYLVABELLE; and, laſtly, a new ſolution of a problem that occurs in computing the orbits of comets. If you think that theſe obſervations do in any way deſerve the notice of the Royal Society, I ſhall be very glad you would communicate them. In order to aſcertain the going of the pendulum clock, I took ſeveral correſponding altitudes of the ſun, which you will find in the following table. On the day of the eclipſe the ſky was very ſerene, nothing could be finer, and it continued ſo during the obſervation. I determined to uſe an achromatic teleſcope of $3\frac{1}{2}$ feet length, that ſhews objects in their natural poſition, becauſe the diluted and uncertain termination of the true ſhadow of the earth appears more perfectly defined by ſmall than by

large teleſcopes, which magnify too much, and give too great a tranſit between the penumbra and the true dark ſhadow. On that account ſome celebrated aſtronomers adviſe to uſe for the eclipſes of the moon no greater teleſcopes than of four or five feet length. It was remarked at Paris, that in an eclipſe of the moon, obſerved through a teleſcope of DOLLOND, the focus of its object lens being 30 inches, and likewiſe through a teleſcope of five feet length; the eclipſe appeared to begin 4′ 7″ ſooner, and to end 4′ 7″ later, through the ſmall than through the long teleſcope; the like has been remarked by ſeveral others, and it has been alſo obſerved by myſelf. As to my obſervations I am tolerably ſatisfied with them, as they do not differ materially from thoſe of Father LE FEVRE, though it is known that in eclipſes of the moon no greater exactneſs than that of a minute can be obtained. The moon's ſpots were carefully obſerved; for it is known, that the mean of the obſervations of the moon's ſpots is ſufficient to aſcertain the longitude of a place to 4″ or 5″ nearly. M. DE LA LANDE comparing the obſervations of the moon's ſpots in an eclipſe, made the 22d of November, 1760, in Vienna, by the Imperial Aſtronomer Abbé HELL, with thoſe made at the ſame time in Paris by M. MESSIER, finds the difference of meridians to be 56′ 13″, which agrees very exactly with that aſcertained by other means.

Correſpondent

Correſpondent altitudes of the Sun taken with a quadrant of three-feet radius, in order to adjuſt the pendulum clock to apparent time.

18th March, 1783.	Sun's altit.	Iſt obſervation.	Sun's altit.	IId obſervation.	Sun's altit.	IIId obſervation.	Sun's altit.	IVth obſervation.	Sun's altit.	Vth obſervation.
The ſun's upper limb at the horizontal wire, eaſtern ſide	° ′ 25 40	h. ′ ″ 8 25 2	° ′ 26 10	h. ′ ″ 8 28 25	° ′ 26 40	h. ′ ″ 8 31 43	° ′ 27 20	h. ′ ″ 8 36 16	° 28	h. ′ ″ 8 40 53
Sun's upper limb at the ſame altitude, weſtern ſide		15 12 24		15 9 1		15 5 39		15 1 10		14 56 29
Dividing the ſum by 2		23 37 26		23 37 26		23 37 22		23 37 26		23 37 22
Sun's center on the meridian as marked by the clock		11 48 43		11 48 43		11 48 41		11 48 43		11 48 41
Equation of the day		12 8 15		12 8 15		12 8 15		12 8 15		12 8 15
Clock ſlower than equated ſolar time		0 19 32		0 19 32		0 19 34		0 19 32		0 19 34

The mean of which I put 19′ 32″

19th March, 1783.	Sun's Altit.	Ist obſervation.	Sun's Alt.	IId obſervation.
The ſun's upper limb at the horizontal wire of the firſt teleſcope on the eaſtern ſide - - -	° ′ 31 30	h. ′ ″ 9 1 56	° 33	h. ′ ″ 9 13 9
Sun's upper limb at the ſame altitude on the weſtern ſide of the meridian -		14 31 47		14 20 33
Dividing the ſum by 2 - -		23 33 43		23 33 42
Sun's center on the meridian as marked by the pendulum clock - -		11 46 51½		11 46 51
Equation of the day		12 7 57		12 7 57
Clock ſlower than equated ſolar time		0 21 6		0 21 6

Clock ſlower than equated ſolar time	19th March	21′ 6″
—— —— ——	18th March	19 32
Retarding of the clock upon 23 h. 58′ 8″ -		1 34

I obſerved too the mid-day at the great gnomon of the obſervatory, and found at the ſame time the meridian line erroneous by 19″, as you will find in the following tables.

		h. ′ ″		h. ′ ″		h. ′ ″
When the center of the ſun's image was on the meridian the time pointed by the clock was	the 17th Mar.	11 50 50	the 18th Mar.	11 48 56	the 19th Mar.	11 47 3
Equations of thoſe days -		12 8 33		12 8 15		12 7 57
Retarding on equated ſolar time		17 43		19 19	the	20 54
Retarding the 17th - -				17 43	18th	19 19
Retarding of the clock during thoſe 24 hours				1 36		1 35

I fixed therefore the retarding of the clock 1′ 35″.

True

		h.	′	″		h.	′	″
True mid-day concluded by the ſun's correſpondent altitudes as the clock marked	the 18th Mar.	11	48	42	the 19th Mar.	11	46	51
Equation of the mid-day - -			—	18			—	18
Retarding of the clock at the rate of 1′ 35″ per 24 hours - -			+	13			+	11
True mid-day the pendulum clock ſhould have marked - - -		11	48	37		11	46	44
Mid-day concluded at the gnomon of the obſervatory - - - -		11	48	56		11	47	3
Difference, the error of the meridian line or gnomon - - -				19				19

From thence I concluded,

	h.	′	″		h.	′	″
Mid-day at true ſolar time	11	59	60	Mid-day at equated ſolar time	12	8	15
Mid-day the clock ſhould have marked on the 18th	11	48	37	Mid-day the clock ſhould have marked on the 18th	11	48	37
Retarding upon true ſolar time - -		11	23	Retarding upon equated ſolar time -		19	38

Obſervations

Obſervations of the moon's eclipſe the 18th March, 1783.

My obſervations with an achromatic teleſcope of $3\frac{1}{2}$ feet length.	Time marked by the clock.	True or apparent time.
IMMERSIONS.		
The beginning of the eclipſe very doubtful.	h. ′ ″	h. ′ ″
Shadow touches Grimaldi - -	7 41 45	7 53 39
Grimaldi all in the ſhadow - -	7 42 54	7 54 48
Shadow touches Mare Humorum - -	7 49 32	8 1 27
——— Copernicus - -	8 0 21	8 12 16
Copernicus all in the ſhadow - -	8 2 29	8 14 24
Tycho touches the ſhadow - -	8 6 18	8 18 13
Eudoxus ——— - -	8 12 19	8 24 15
——— all in the ſhadow - -	8 13 43	8 25 39
Mare Serenitatis touches the ſhadow -	8 16 7	8 28 3
——— the ſhadow in the middle -	8 21 31	8 33 27
——— all in the ſhadow - -	8 25 54	8 37 50
Proclus touches the ſhadow - -	8 32 21	8 44 18
Mare Criſium touches the ſhadow -	8 33 29	8 45 26
——— ſhadow in the middle -	8 35 36	8 47 33
——— all in the ſhadow - -	8 36 56	8 48 53
Total immerſion - - -	8 38 57	8 50 55
EMERSIONS.		
Beginning of the emerſion - -	10 19 57	10 32 2
Grimaldi emerging - -	10 23 33	10 35 38
——— all out of the ſhadow - -	10 24 9	10 36 14
Mare Humorum emerging - -	10 29 34	10 41 39
Total emerſion of Mare Humorum -	10 35 37	10 47 43
Copernicus all out of the ſhadow -	10 43 6	10 55 12
Mare Serenitatis all emerged - -	10 57 32	11 9 39
Mare Criſium all emerged - -	11 15 44	11 27 51
End of the eclipſe - -	11 20 10	11 32 18
Total duration - - -		3 39 0

Father

Father LE FEVRE's obſervations with a reflector 55 inches focal length, magnifying 300 times.

	Time by the clock.	Apparent time.
IMMERSIONS.	h. ′ ″	h. ′ ″
Grimaldi touches the ſhadow - -	7 41 43	7 53 37
Kepler touches the ſhadow - -	7 52 2	8 3 57
—— all in the ſhadow - -	7 53 24	8 5 19
Copernicus touches the ſhadow - -	8 0 22	8 12 17
—— all in the ſhadow - -	8 2 26	8 14 21
Mare Serenitatis touches the ſhadow -	8 16 7	8 28 3
—— all in the ſhadow - -	8 26 2	8 37 58
Mare Criſium touches the ſhadow - -	8 33 28	8 45 25
—— all in the ſhadow - -	8 36 56	8 48 53
Total immerſion - - -	8 38 54	8 50 52
EMERSIONS.		
Beginning of the emerſion - -	10 19 42	10 31 47
Grimaldi emerged - -	10 23 24	10 35 29
Kepler all out of the ſhadow - -	10 35 43	10 47 49
Copernicus all out - - -	10 43 4	10 55 10
Mare Serenitatis all out - - -	10 57 19	11 9 26
—— Criſium all out - - -	11 15 50	11 27 57
End of the eclipſe - - -	11 20 22	11 32 30
Total duration - - -		3 39 20

The obſervation of the vernal equinox was made at the gnomon. The height of this gnomon, taken from the center of the hole by which the beams of the ſun come in, is 1878 lines of a French inch; the diſtance from the bottom of the gnomon to the equinoctial point is 1928; the diſtance from the upper limb of the ſun's image to the equinoctial point was found 16,7; the diſtance from the under limb 23,4; the diameter of the hole = 6; therefore the diſtance from the bottom to the upper limb 1928 − 16,7 + 3 = 1914,3, to the under limb 1928 + 23,4 − 3 = 1948,4; which gives the time the equinox happened the 20th of March, 5 h. 56′ 52″ P.M.

Obſervations of Jupiter's ſatellites at Marſeilles.

1782		Apparent time.	Obſervation.
		h. ′ ″	
April 3	Immerſion of the Iſt ſatellite at -	2 22 56	good
May 19	Imm. Iſt ſat. - - -	2 48 12	good
June 7	Imm. IVth ſat. was not total, but its light diminiſhed ſenſibly without ever diſappearing; the ſky was ſerene, and Jupiter had ſix belts very diſtinctly.		
20	Emerſion of the Iſt ſatellite - - -	1 29 46	good
July 5	Em. Iſt ſat. - - -	11 43 59	good
13	Em. IId ſat. - - -	9 17 28	good
20	Em. IIId ſat. - - -	9 27 13	good
20	Em. IId ſat. - - -	11 51 59	good
21	Em. Iſt ſat. - - -	10 1 11	good
27	Imm. IIId ſat. - - -	10 40 33	good
Aug. 6	Em. Iſt ſat. - - -	8 21 20	good
13	Em. Iſt ſat. - - -	10 18 49	good
14	Em. IId ſat. - - -	8 55 34	good
Sept. 1	Em. IIId ſat. - - -	9 40 44	doubtful
14	Em. Iſt ſat. - - -	8 6 48	good

IT is known, that the indirect method to calculate the orbits of comets in a conic ſection, by means of three obſervations given, is rendered more eaſy and expeditious if there is a poſſibility of drawing a graphical figure that repreſents nearly the orbit under conſideration, by means of which the calculation is directed, and the required elements of the comet's path may be rigorouſly determined. To draw the orbit of a comet that moves in a parabola or ellipſis, the problem is reduced to find the poſition of the axis and the perihelial diſtance; this poſition of the axis will be determined as ſoon as the angle is known, that the axis forms with another line, whoſe poſition is given; this line may be an ordinate to a given point of the curve, or a tangent, or a radius vector, &c. The latter is to

be employed in preference, becauſe the perihelial diſtance being a conſtant quantity, the angle of poſition then becomes the true anomaly of the comet; but as the data of this problem are only geocentric longitudes and latitudes of the comet, deduced from the immediate obſervations of right aſcenſion and declination, the heliocentric longitudes and latitudes muſt firſt be calculated; but as thoſe data are not ſufficient, what is not given muſt be arbitrarily ſuppoſed, *viz.* the ſhortened diſtances (*diſtantias curtatas*). This ſuppoſition is changed and altered until the calculation will agree with the three obſervations, then the difference between two longitudes is the angle comprehended between the two ſhortened diſtances in the plane of the ecliptic; the whole reduced to the plane of the comet's orbit by means of the heliocentric latitude, gives the difference between the anomalies comprehended by two radius vectors, the problem then is reduced: two radius vectors being given, with the angle comprehended, to find the two true anomalies, the perihelial diſtance, and the time the comet puts in running its anomalies.

Let therefore ♈ ♋ ♎ ♑ repreſent the ecliptic at an infinite diſtance; QPR the apparent elliptical or parabolical path of a comet; S the ſun's center; P the comet's perihelion; T the place of the earth when the comet was firſt obſerved in C; I the earth's place when the comet was obſerved in K; ST = d, SI = δ, the diſtances from the earth to the ſun at the firſt and ſecond obſervation known by aſtronomical tables; let Cm and Kn be two perpendiculars to the plane of the ecliptic, it will be Sm = u, Sn = v the two ſhortened diſtances.

The obſerved geocentric longitude of the comet in T = a = arc ♈ ♑ G;
the obſerved geocentric longitude of the comet in I = α = arc ♈ ♑ H;
the geocentric longitude of the ſun by tables in T = b = arc ♈ ♑ ♎ A;
the geocentric longitude of the ſun by tables in I = β = arc ♈ ♑ ♎ B.

Now for the firſt obſervation the angle of elongation is $b-a$; for the angle ATG = arc AG = ♈ ♑ = A − ♈ ♑ G = long. ☉ − long. comet $= b-a$;

the angle of the annual parallax $SmT = \frac{\text{ſin.}(b-a)d}{u} = e$;

the angle of commutation $mST = {}^{0°}_{180°} - e + (b-a) = f$;

from whence the heliocentric longitude of the comet = $b - 180° + f = g$.

The ſame at the ſecond obſervation in I.

Angle of elongation $= \beta - \alpha$;

Angle of annual parallax $\varepsilon = \frac{\text{ſin.}(\beta-\alpha)\delta}{v}$;

Angle of commutation $\varphi = {}^{0}_{180°} - \varepsilon + (\beta - \alpha)$;

heliocentric longitude of the comet in I $= \beta - 180° + \varphi = \gamma$;

putting now the heliocentric latitude ſeen from S $= k$;

the geocentric latitude ſeen from T $= l$;

the heliocentric latitude will be $\frac{\text{ſin. } f \text{ . tang. } l}{\text{ſin. } (b-a)} = \text{tang. } k$;

the ſame with Kn it will be $\frac{\text{ſin } \varphi \text{ . tang. } \lambda}{\text{ſin. } (\beta-\alpha)} = \text{tang. } \varkappa$ heliocentric latitude in K.

Having thus determined the heliocentric latitudes of two obſervations, the radius vectors will eaſily be found in the ſuppoſition made for the ſhortened diſtances, for they are in the ſame ratio to the radius vectors as the coſine of the heliocentric latitudes are to the radius = 1; therefore the radius vector m of the firſt obſervation will be $= \frac{u}{\text{cof. } k}$, and the radius vector of the ſecond obſervation $\mu = \frac{v}{\text{cof. } \varkappa}$.

Taking now the difference between the found heliocentric longitudes, we get the heliocentric motion of the comet upon the ecliptic between two ſhortened diſtances, which is to be reduced upon the comet's orbit, this heliocentric motion is therefore $\gamma - g = m$. Now to reduce this motion we have, firſt,

ſinus

ſinus totus = 1 is to coſine m :: as cotangent k is to the tangent of an angle which I put = n, and $90° - k \mp n$ will give an angle which I put = q. Laſtly, the analogy coſ. n : coſ. q :: ſin. k : will give the coſine of an angle ψ, which is the required motion upon the orbit, or the angle comprehended between the two radius vectors m and μ. Let therefore ECPMND be the apparent parabolic path of a comet; S the ſun's center; M and N two places of the comet, the angle MSN equal to its motion in longitude, or the comprehended angle ψ; P the perihelion; it is required to find the two anomalies PM, PN, that is, PSM and PSN, the perihelial diſtance SP, and the time the comet employed to come from its perihelion P to M and N.

Reſolution.

SM = m
SN = μ
MSN = ψ
NSB = x
MSB = $(\psi \pm x)$
Parameter = p

In the right-angled triangle SMR and SNV we have MR = OS = m ſin. $(\psi \pm x)$ NV = QS = μ ſin. x; therefore OP = $\frac{1}{4}p - m$ (ſin. $\psi \pm x$) and PQ = $\frac{1}{4}p \mp \mu$ ſin. x; but by the nature of the parabola we have SM = AP + PO and SN = AP + PQ; that is

$m = \frac{1}{2}p - m$ (ſin. $\psi \pm x$) $\quad \mu = \frac{1}{2}p \mp \mu$ ſin. x

$m + m$ (ſin. $\psi \pm x$) = $\frac{1}{2}p$ $\quad \mu \pm \mu$ ſin. $x = \frac{1}{2}p$

m (1 + ſin. $\psi \pm x$) = $\frac{1}{2}p$ $\quad \mu$ (1 $\mp$ ſin. x) = $\frac{1}{2}p$

and 1 + ſin. $(\psi \pm x) = \frac{p}{2m}$ $\quad 1 \pm$ ſin. $x = \frac{p}{2\mu}$; by putting into a ſum 1 + ſin. $(\psi \pm x)$ + 1 $\pm$ ſin. $x = \frac{p}{2m} + \frac{p}{2\mu}$; reduction made $2 \pm$ ſin. x + ſin. $(\psi \pm x) = \left(\frac{m+\mu}{2m\mu}\right) p$; but by trigonometrical formulæ we have ſin. $(\psi \pm x)$ = ſin. ψ coſ. $x \pm$ ſin. x coſ. ψ. Subſtituting this expreſſion in its place we obtain, $2 \pm$ ſin. x + ſin. ψ coſ. $x \pm$ ſin. x coſ. $\psi = \left(\frac{m+\mu}{2m\mu}\right) p$. By the ſame formulæ we have coſ.^{2} $x = 1 -$ ſin.^{2} x and coſ. $x = \sqrt{1 -$ ſin.^{2} $x}$.

Subſtituting it comes out, $2 \pm \text{ſin. } x + \text{ſin. } \psi \sqrt{1 - \text{ſin.}^2 x} \pm \text{ſin. } x \text{ coſ. } \psi = \left(\frac{m+\mu}{2m\mu}\right) p$ and $\text{ſin. } \psi \sqrt{1 - \text{ſin.}^2 x} = \left(\frac{m+\mu}{2m\mu}\right) p - 2 \mp \text{ſin. } x \mp \text{ſin. } x \text{ coſ. } \psi$. By caſting away the ſign of the ſquare root, and reducing to o,

$$\left(\frac{m+\mu}{4m^2\mu^2}\right)^2 p^2 + 4 + \text{ſin.}^2 x + \text{ſin.}^2 x \text{ coſ.}^2 \psi - 2\left(\frac{m+\mu}{m\mu}\right) p \mp \left(\frac{m+\mu}{m\mu}\right) p \text{ . ſin. } x \mp \left(\frac{m+\mu}{m\mu}\right) p \text{ . (ſin. } x \text{ coſ. } \psi)$$

$$\mp 4 \text{ ſin. } x \text{ coſ. } \psi + 2 \text{ ſin.}^2 x \text{ coſ. } \psi - \text{ſin.}^2 \psi + \text{ſin.}^2 x \text{ ſin.}^2 \psi = 0 \text{ ; diſentangling the equation,}$$

$$\frac{m^2p^2 + 2m\mu p^2 + \mu^2 p^2}{4m^2\mu^2} + 4 + \text{ſin.}^2 x + \text{ſin.}^2 x \text{ coſ.}^2 \psi - \frac{2mp - 2\mu p}{m\mu} \mp \frac{mp \text{ ſin. } x \mp \mu p \text{ ſin. } x}{m\mu} \mp \frac{mp \text{ ſin. } x \text{ coſ. } \psi \mp \mu p \text{ ſin. } x \text{ coſ. } \psi}{m\mu}$$

$$\pm 4 \text{ ſin. } x \pm 4 \text{ ſin. } x \text{ coſ. } \psi + 2 \text{ ſin.}^2 x \text{ coſ. } \psi - \text{ſin.}^2 \psi + \text{ſin.}^2 x \text{ ſin.}^2 \psi = 0. \text{ But } p \text{ being equal to}$$

$2\mu \pm 2\mu \text{ ſin. } x$, ſubſtituting this value, we have

$$\frac{4m^2\mu^2 \pm 8m^2\mu^2 \text{ ſin. } x + 4m^2\mu^2 \text{ ſin.}^2 x + 8m\mu^3 + 8m\mu^3 \text{ ſin.}^2 x \pm 16m\mu^3 \text{ ſin. } x + 4\mu^4 + 4\mu^4 \text{ ſin.}^2 x \pm 8\mu^4 \text{ ſin. } x}{4m^2\mu^2} + 4 + \text{ſin.}^2 x +$$

$$+ \text{ ſin.}^2 x \text{ coſ.}^2 \psi - \frac{4m\mu \mp 4m\mu \text{ ſin. } x - 4\mu^2 \mp 4\mu^2 \text{ ſin. } x \mp 2m\mu \text{ ſin. } x - 2m\mu \text{ ſin.}^2 x \mp 2\mu^2 \text{ ſin. } x - 2\mu^2 \text{ ſin}^2 x \mp}{m\mu}$$

$$\mp 2m\mu \text{ ſin. } x \text{ coſ. } \psi - 2m\mu \text{ ſin.}^2 x \text{ coſ. } \psi \mp 2\mu^2 \text{ ſin. } x \text{ coſ. } \psi - 2\mu^2 \text{ ſin.}^2 x \text{ coſ. } \psi \pm 4 \text{ ſin. } x \pm 4 \text{ ſin. } x \text{ coſ. } \psi + 2 \text{ ſin.}^2 x \text{ coſ. } \psi - \text{ſin.}^2 \psi +$$

$$+ \text{ſin.}^2 x \text{ ſin.}^2 \psi = 0. \text{ Dividing the numerator and denominator of the firſt fraction by } 4\mu^2,$$

and of the ſecond by μ, and putting the whole to the common denominator m^2 it comes

$$\frac{m^2 \pm 2m^2 \text{ ſin. } x + m^2 \text{ ſin.}^2 x + 2m\mu + 2m\mu \text{ ſin.}^2 x \pm 4m\mu \text{ ſin. } x + \mu^2 + \mu^2 \text{ ſin}^2 x \pm 2\mu^2 \text{ ſin. } x + 4m^2 + m^2 \text{ ſin.}^2 x \text{ coſ.}^2 \psi + m^2 \text{ ſin.}^2 x -}{m^2}$$

$$- 4m^2$$

$$\frac{-4m^2\mp 4m^2\,\text{sin.}\,x\mp 4m\mu\mp 4m\mu\,\text{sin.}\,x\mp 2m^2\,\text{sin.}\,x-2m^2\,\text{sin.}^2x\mp 2m\mu\,\text{sin.}\,x-2m\mu\,\text{sin.}^2x\mp 2m^2\,\text{sin.}\,x\,\text{cos.}\,\psi-2m^2\,\text{sin.}^2x\,\text{cos.}\,\psi\mp}{m^2}$$

$$\frac{\mp 2m\mu\,\text{sin.}\,x\,\text{cos.}\,\psi-2m\mu\,\text{sin.}^2x\,\text{cos.}\,\psi\pm 4m^2\,\text{sin.}\,x\pm 4m^2\,\text{sin.}\,x\,\text{cos.}\,\psi+2m^2\,\text{sin.}^2x\,\text{cos}\,\psi-m^2\,\text{sin.}^2\psi+m^2\,\text{sin.}^2x\,\text{sin.}^2\psi}{m^2}=0.$$

Casting away m^2, and reducing, we have

$m^2+\mu^2+\mu^2\,\text{sin.}^2x\pm 2\mu^2\,\text{sin.}\,x+m^2\,\text{sin.}^2x\,\text{cos.}^2\psi-2m\mu\mp 2m\mu\,\text{sin.}\,x\,\text{cos.}\,\psi-2m\mu\,\text{sin.}^2x\,\text{cos.}\,\psi-m^2\,\text{sin.}^2\psi+m^2\,\text{sin.}^2x\,\text{sin.}^2\psi\mp$
$\mp 2m\mu\,\text{sin.}\,x\pm 2m^2\,\text{sin.}\,x\,\text{cos}\,\psi=0.$

Putting together sin.^2x and $\text{sin.}\,x$

$(\mu^2+m^2\text{cos.}^2\psi-2m\mu\,\text{cos.}\,\psi+m^2\,\text{sin.}^2\psi)\,\text{sin.}^2x\pm(2\mu^2\mp 2m\mu\,\text{cos.}\,\psi\mp 2m\mu\pm 2m^2\,\text{cos.}\,\psi)\,\text{sin.}\,x+m^2-2m\mu+\mu^2-m^2\,\text{sin.}^2\psi=0.$

Substituting $\text{cos.}^2\psi=1-\text{sin.}^2\psi$ we recover a quadratic equation,

$$(\mu^2+m^2-2m\mu\text{cos.}\psi)\text{sin.}^2x\pm(2\mu^2\mp 2m\mu\text{cos.}\psi\pm 2m^2\text{cos.}\psi\mp 2m\mu)\text{sin.}\,x+m^2-2m\mu+\mu^2-m^2\text{sin.}^2\psi=0,$$

and $\text{sin.}^2x=\pm\dfrac{(2\mu^2\mp 2m\mu\,\text{cos.}\,\psi\pm 2m^2\text{cos.}\,\psi\mp 2m\mu)}{(\mu^2+m^2-2m\mu\,\text{cos.}\,\psi)}\,\text{sin.}\,x=\dfrac{2m\mu-m^2-\mu^2+m^2\,\text{sin}^2\psi}{\mu^2+m^2-2m\mu\,\text{cos.}\,\psi}.$

This equation resolved gives

$$\text{sin.}\,x=\pm\frac{\mu^2\mp m\mu\,\text{cos.}\,\psi\pm m^2\,\text{cos.}\,\psi\mp m\mu}{\mu^2+m^2-2m\mu\,\text{cos.}\,\psi}=\sqrt{\frac{2m\mu-m^2-\mu^2+m^2\,\text{sin.}^2\psi}{\mu^2+m^2-2m\mu\,\text{cos.}\,\psi}\pm\frac{(\mu^2\mp m\mu\,\text{cos.}\,\psi\pm m^2\,\text{cos.}\,\psi\mp m\mu)^2}{(\mu^2+m^2-2m\mu\,\text{cos.}\,\psi)^2}}$$ which gives farther,

$$\text{sin.}\,x=\frac{\sqrt{(2m\mu-m^2-\mu^2+m^2\text{sin.}^2\psi)\,.\,(\mu^2+m^2-2m\mu\text{cos.}\psi)}\pm(\mu^2\mp m\mu\text{cos.}\psi\pm m^2\text{cos.}\psi\mp m\mu)^2}{\mu^2+m^2-2m\mu\,\text{cos.}\,\psi}\mp\frac{\mu^2\mp m\mu\text{cos.}\psi\pm m^2\text{cos.}\psi\mp m\mu}{\mu^2+m^2-2m\mu\,\text{cos.}\,\psi}.$$

Putting $1-\text{sin.}^2\psi$ instead of $\text{cos.}^2\psi$, after all reduction made, we recover

$$\text{sin.}\,x=\frac{\sqrt{2m^3\mu\,\text{sin}^2\psi-2m^3\mu\,\text{cos.}\,\psi\,\text{sin.}^2\psi}}{m^2+\mu^2-2m\mu\,\text{cos.}\,\psi}\pm\left(\mp\frac{\mu^2\pm\mu m\,\text{cos.}\,\psi\mp m^2\,\text{cos.}\,\psi\pm m\mu}{\mu^2+m^2-2m\mu\,\text{cos.}\,\psi}\right);\ \text{that is,}$$

$\text{sin.}\,x=(\pm\mu\mp m)\,.\,(m\,\text{cos.}\,\psi-\mu)\pm\sqrt{2m^3\mu\,\text{sin.}^2\psi\,(1-\text{cos.}\,\psi)}$, which gives at last the simple expression $\text{sin.}\,x=\dfrac{(\pm\mu\mp m)\,.\,(m\,\text{cos.}\,\psi-\mu)\pm m\,\text{sin.}\,\psi\sqrt{2m\mu\,(1-\text{cos.}\,\psi)}}{m^2+\mu^2-2m\mu\,\text{cos.}\,\psi}.$

The angle x defines therefore the poſition of the axis and the two anomalies required, the perihelial diſtance being $p = 2\mu \pm 2\mu$ ſin. x, it will be known alſo by the angle x.

In order to find the time the comet employs in running its anomalies, let the perihelial diſtance juſt now inveſtigated p be equal to the radius of the earth's orbit, the parabolic area ſwept by the radius vector will be by the nature of the parabola $\frac{2}{3}$ PO × OM + $\frac{1}{2}$ SO × OM = $\frac{4\text{PO} \times \text{OM} + 3\text{SO} \times \text{OM}}{6}$. Now the periphery of the earth's orbit is $7 : 22 :: 2p : \frac{44}{7}p$; therefore the whole area $\frac{44}{7}p \cdot \frac{1}{2}p = \frac{22}{7}p^2$. It is known that the velocity of a heavenly body moved in a circular path, is to the velocity in a parabolic path in the ratio $\sqrt{2} : 1$. If the parabolic area of the comet is divided by $\sqrt{2}$ it comes out $\frac{4\text{PO} \times \text{OM} + 3\text{SO} \times \text{MO}}{6\sqrt{2}}$ equal to an area that the earth deſcribes in the very ſame time; put therefore A equal to the time of a ſidereal year, we ſhall recover the analogy; the whole area of the earth's orbit is to the time in which it is deſcribed as the parabolic area is to the time conſumed in ſweeping it; therefore $\frac{22}{7}p^2 : \text{A} :: \frac{(4\text{PO} + 3\text{SO})\ \text{MO}}{6\sqrt{2}} : \frac{7\text{A}\ (4\text{PO} + 3\text{SO})\ \text{MO}}{72p^2\sqrt{2}}$; but OM = SM . ſin. anom. PSM and OS = SM . coſ. anom. PSM; let the anomaly be = δ, we have OM = m ſin. δ, and OS = m coſ. δ; therefore PO = $p - m$ coſ. δ. Subſtituting we obtain $\frac{7\text{A}\ (4p - 4m \text{ coſ. } \delta + 3m \text{ coſ. } \delta)\ m \text{ ſin. } \delta}{72p^2\sqrt{2}}$ which is $\frac{7\text{A}\ (4p - m \text{ coſ. } \delta)\ m \text{ ſin. } \delta}{72p^2\sqrt{2}}$, whereby the time is found in parts of a ſidereal year.

I am, &c.

SIR,

SIR, Lyons, May 4, 1783.

LATELY I received from the Obſervatory at Marſeilles the obſervation of the tranſit of Mercury, which happened the 12th Nov. 1782. The ſky not being very favourable, only the two internal contacts were obſerved; the firſt internal contact was obſerved by M. ST. JACQUES DE SYLVABELLE, at 3 h. 18′ 30″ apparent time; the laſt internal contact by M. ST. JACQUES, at 4 h. 30′ 16″; by M. BERNARD, his Adjunctus, at 4 h. 29′ 13″. The neareſt diſtances of Mercury's limb to that of the ſun in the northern part of its diſk were at

h.	′	″		
3	33	14	31	} parts of the micrometer.
3	42	57	34	
4	22	17	19	

The apparent diameter of the ſun was 2174 parts of this micrometer: I ſuppoſe the before-mentioned 2174 parts = 32′ 26″,9. I conclude farther, by the obſervations, the middle of the tranſit at 3 h. 54′ 7″,25, whereas I fix, by interpolation, the diſtances of the limbs at 3 h. 54′ 7″,25 = 35″,6; I have therefore ſemi-diameter of the ſun = 16′ 13″,4 − 35″,6 = 15′ 37″,8 + ſemi-diameter of Mercury = 6″ = 15′ 43″,8 = to the leaſt diſtance of centers of the ſun and Mercury. By M. DE LA LANDE's tables it is 15′ 42″, only a difference of 1″,8.

M. WALLOT at Paris has obſerved this tranſit at the Royal Obſervatory,

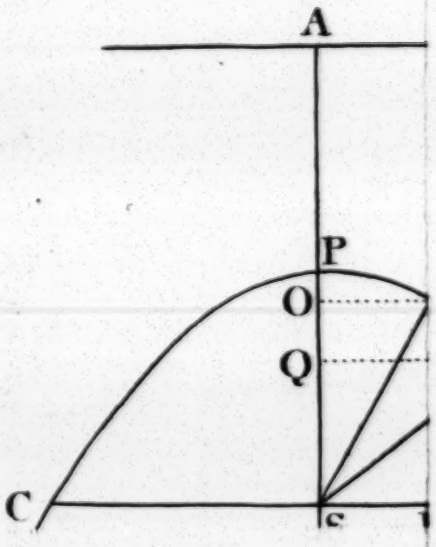
	h.	′	″
Firſt external contact	2	56	28
Firſt internal contact	3	2	3
Second - -	4	17	18
Second external -	4	22	53

I only add an important remark upon the diameter of Mercury, which the aſtronomers ſuppoſed in this tranſit = 12″.

Let ABC repreſent the ſun's diſk; in P an external in Q an internal contact; ANC the apparent path of Mercury over the ſun.

The ſemi-diameter of the ſun = 972″, this of Mercury in our ſuppoſition = 6″, MN = 942″ the leaſt diſtances of the centers. In the right-angled triangle MNP it is MP = 972″ + 6″ = 978″, MQ = 972″ − 6″ = 966″; therefore NP will be found = 260″ and NQ = 210″: now NP − NQ = PQ = 50″, which converted into time gives 8′ 14″ for the time the diameter of Mercury employed to run over the limb of the ſun; but by the obſervations of M. WALLOT I find this time conſtantly in both contacts 5′ 35″; therefore 8′ 14″ : 12″ :: 5′ 35″ : 8″,137, which ſhould be the diameter of Mercury; and indeed M. WALLOT, by an immediate meaſure, taken with an excellent wire-micrometer, finds this apparent diameter not greater than 9″, which ſufficiently ſhews that this diameter ſuppoſed 7″ in the mean diſtance is alſo too great.

I am, &c.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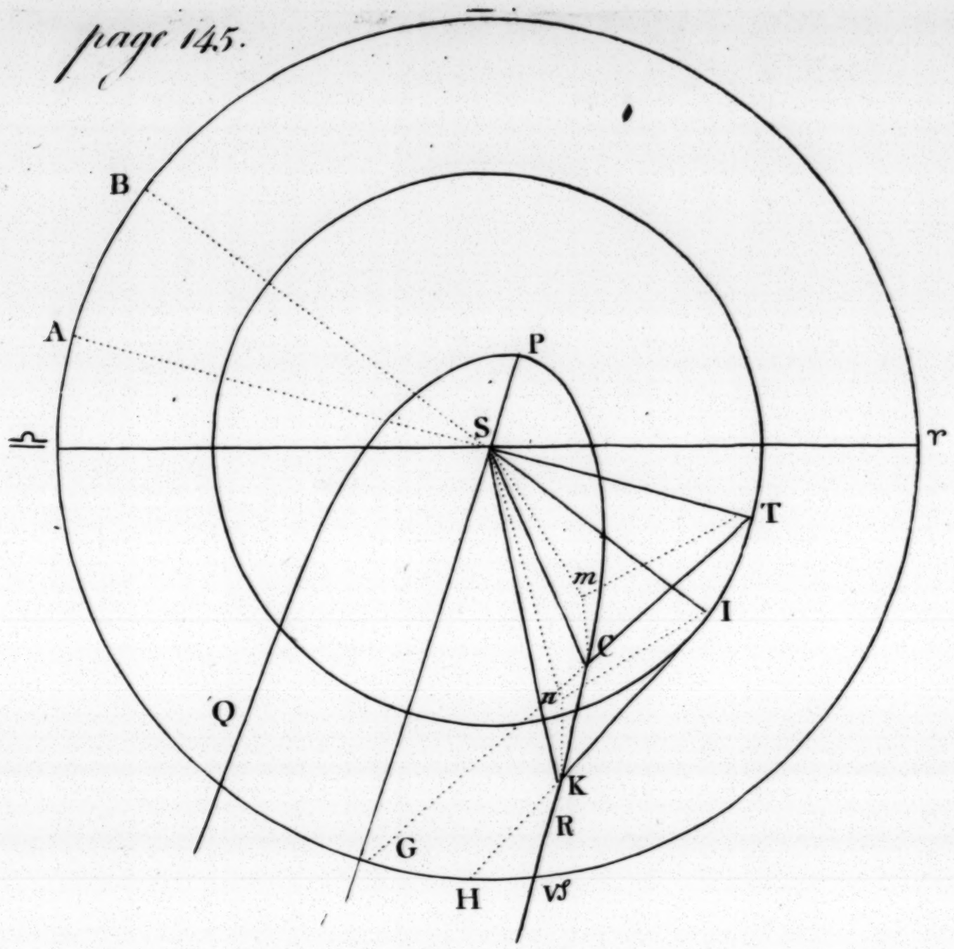

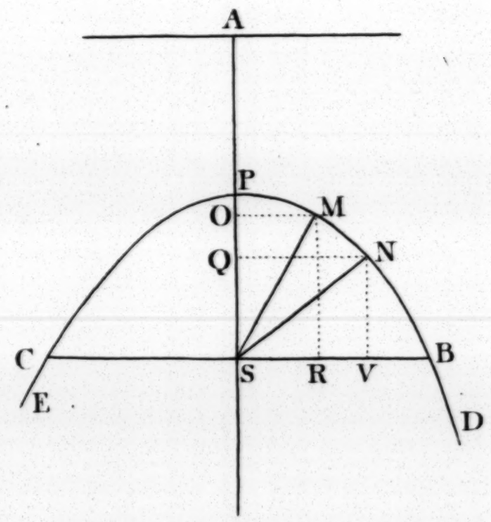

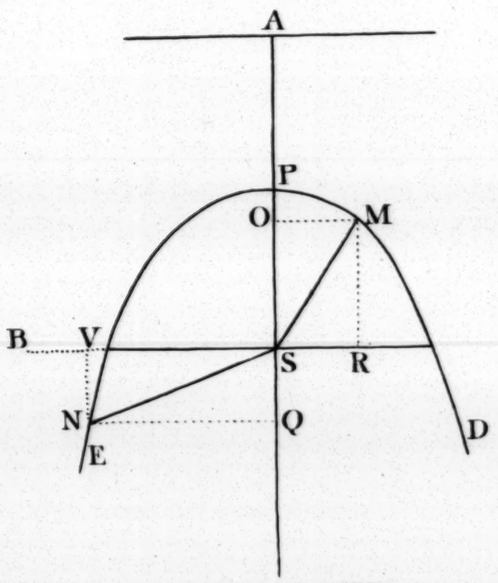

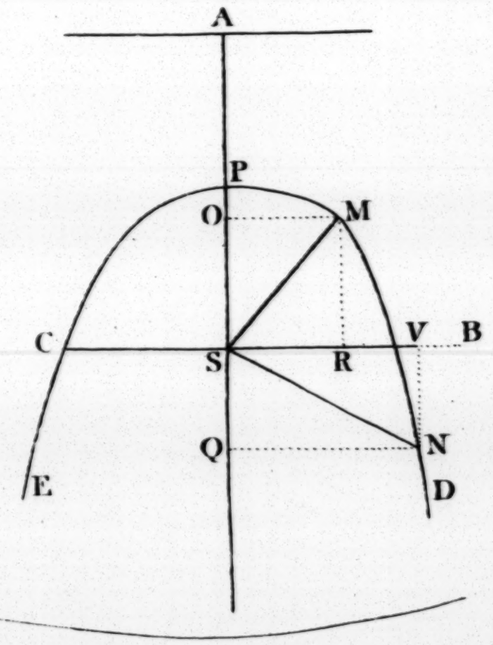

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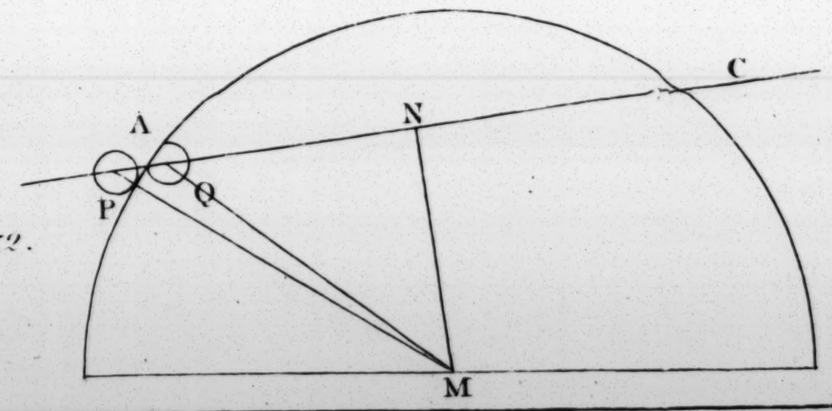

IX. *Obſervations of a new Variable Star.* *By* John Goodricke, *Eſq.*; *communicated by* *Sir* H. C. Engleſield, *Bart. F. R. S. and A. S.*

Read January 27, 1785.

TO SIR H. C. ENGLEFIELD, BART.

DEAR SIR, York, Jan. 10, 1785.

THE account that has been lately given of the regular variation of Algol's light, and the notice aſtronomers have been pleaſed to take of it, are well known. It is natural therefore to ſuppoſe, that the relation of other ſimilar phænomena may alſo meet with the ſame favourable reception. Of this kind is the following, which I beg the favour of you to preſent to the Royal Society.

On the 10th of September, 1784, whilſt my attention was directed towards that part of the heavens where β Lyræ was ſituated, I was ſurpriſed to find this ſtar much leſs bright than uſual, whereupon I ſuſpected that it might be a variable ſtar: my ſuſpicions were afterwards confirmed by a ſeries of obſervations, which have been regularly continued ſince that time, and which will preſently follow in their proper place. At firſt I thought the light of this ſtar ſubject to a periodical variation of nearly *ſix days and nine hours*, though the degree of its diminution did not then appear to be conſtant; but now, upon a more cloſe examination of the obſervations themſelves, I am

inclined to think, that the extent of its variation is *twelve days and nineteen hours*, during which time it undergoes the following changes.

1. It is of the third magnitude for about two days.

2. It diminiſhes in about one day and a quarter.

3. It is between the fifth and fourth magnitude for leſs than a day.

4. It increaſes in about two days.

5. It is of the third magnitude for about three days.

6. It diminiſhes in about one day.

7. It is ſomething larger than a ſtar of the fourth magnitude for little leſs than a day.

8. It increaſes in about one day and three quarters to the firſt point, and ſo completes a whole period.

Theſe eight points of the variation are perhaps inaccurately aſcertained; and indeed it cannot be expected to be otherwiſe in eſtimations of this nature, where it is very poſſible to err even ſeveral hours.

The relative brightneſs of β Lyræ, at its obſcuration in the third and ſeventh points, is nearly as follows. When in that of the third point, it is leſs than ζ and $\varkappa$, and nearly equal to δ Lyræ; and when in that of the ſeventh point, it is rather leſs than ξ and θ Herculis, and much brighter than ζ, $\varkappa$, and δ Lyræ. At its greateſt brightneſs in the firſt and fifth points, it is ſometimes brighter than γ Lyræ, but leſs than β Cygni, and ſometimes only nearly equal to it; but in thoſe points it ſeems to alter in its brightneſs ſeveral times in the ſame night, and that generally in a pretty conſiderable degree. However, this may perhaps be only owing to ſome fallacy of obſervation; for I have often perceived, that the relative brightneſs of ſtars is affected not only by the different ſtates of the air, but alſo by

their

their change of poſition occaſioned by the earth's diurnal motion, and that particularly in ſtars of a great altitude.

The magnitudes of the ſtars, to which β Lyræ was compared during the progreſs of its variation, are as follows. β Cygni and γ Lyræ of the third magnitude; ξ and θ Herculis of between the fourth and third magnitude; *o* Herculis is ſomething leſs than a ſtar of the fourth magnitude; ζ, ϰ, and δ Lyræ are ſtars of between the fourth and fifth magnitude, if not nearer the fifth. The relative brightneſs of theſe ſtars follows the order in which they are ſet down.

Obſervations of the brightneſs and magnitude of β Lyræ.

1784, Sept. 10. At 11 h. ±, much leſs than γ Lyræ; nearly equal to, if not rather brighter than ζ, ϰ, and δ Lyræ, and not ſo bright as ξ, θ, and *o* Herculis; between the fourth and fifth magnitude.

Sept. 11. At 8½ h. nearly the ſame as it was laſt night, if not brighter; indifferent obſervation.

Sept. 12. At 8½ h. and 9 h. between the third and fourth magnitude; leſs than γ Lyræ, brighter than θ, ξ, and *o* Herculis, and much brighter than ζ, ϰ, and δ Lyræ. Mr. E. Pigott agrees with me nearly.

Sept. 13, 15, 18, 19, and 20. It was at or near its greateſt brightneſs.

Sept. 23. At 7½ h. it was nearly equal to ζ, ϰ, and δ Lyræ, and much leſs than ξ, θ, and *o* Herculis.

At 10½ h. the air being extremely clear, I compared it more attentively to the neighbouring ſtars, and found it as follows: rather a little brighter than δ, a little leſs than ζ, and rather

leſs than $\varkappa$ Lyræ. Mr. E. PIGOTT thought it had rather increaſed from $8\frac{1}{2}$ to 11 h.

Sept. 24. At $13\frac{1}{2}$ h. certainly brighter than it was laſt night, but intervening clouds precluded all further obſervation.

Sept. 28. At 10 h. not quite ſo bright as γ Lyræ, but rather brighter than θ and ξ Herculis. Mr. E. PIGOTT thought it nearly equal to γ Lyræ.

Sept. 29. At $7\frac{1}{2}$ h. not ſo bright as γ Lyræ.

At $8\frac{1}{2}$ h. to $10\frac{1}{2}$ h. nearly equal to ξ and θ Herculis; but if any thing it ſeemed rather leſs than ξ, and rather brighter than θ; about the fourth magnitude.

At $11\frac{1}{4}$ h. to $12\frac{3}{4}$ h. the ſame, if not leſs; I could not compare it well to ξ and θ, becauſe they were low; moon-light, but the air was clear.

Sept. 30. At 7 h. rather leſs than θ, if not equal to it; a little leſs than ξ, and brighter than o Herculis; about the fourth magnitude.

At 11 h. and $12\frac{3}{4}$ h. it ſeemed to be on its increaſe, being for the moſt part larger than ξ and θ Herculis.

Oct. 1 and 2. About its greateſt brightneſs, but leſs than γ Lyræ. Mr. E. PIGOTT thought it brighter on the 2d than on the 1ſt, being on the 2d nearly equal to γ Lyræ.

Oct. 4. At $10\frac{1}{2}$ h. I thought it rather leſs, but the weather was hazy.

Oct. 5. At $6\frac{1}{2}$ h. not ſo bright as ξ and θ Herculis; a little brighter than ζ, and brighter than δ and $\varkappa$ Lyræ; air clear.

At $9\frac{1}{2}$ h. nearly equal to ζ, and a little brighter than δ and $\varkappa$ Lyræ.

At $12\frac{1}{2}$ h. a little leſs than ζ, nearly equal to $\varkappa$, and rather a little brighter than δ Lyræ; between the fourth and fifth magnitude; air very clear.

Oct. 6. At $6\frac{1}{4}$ h. and $7\frac{1}{4}$ h. leſs than ζ and $\varkappa$, and a little leſs than δ Lyræ; between the fifth and fourth magnitude.

Oct. 7. At $6\frac{3}{4}$ h. between the fourth and third magnitude; a little brighter than θ, and nearly equal to ξ Herculis; much brighter than ζ, $\varkappa$, and δ Lyræ; I obſerved it till $12\frac{1}{2}$ h. when it was certainly increaſed.

At $7\frac{1}{2}$ h. Mr. E. PIGOTT thought it brighter than ξ and θ Herculis.

Oct. 8. At 8 h. nearly equal to γ Lyræ; on account of the intervening clouds, I could not perceive which was largeſt; third magnitude.

Oct. 9. At 7 h. rather leſs than γ Lyræ.

Oct. 10. At 7 h. $11\frac{1}{4}$ h. and 12 h. nearly equal to γ, if not rather leſs.

Oct. 11. At 8 h. 10 h. and 12 h. rather leſs than γ; at 12 h. if any difference, leſs than it was laſt night.

Oct. 15. At 8 h. $=$ nearly equal to, though rather leſs than, γ Lyræ.

Oct 16. At $6\frac{1}{2}$ h. and $9\frac{1}{2}$ h. little leſs than γ, if not equal to it.

At 11 h. rather larger than γ, but the weather was foggy. Mr. E. PIGOTT agrees with me in both obſervations.

Oct. 17. At $6\frac{1}{2}$ h. and 7 h. ſomewhat leſs than γ Lyræ.

Oct. 18. At $6\frac{3}{4}$ h. between the fourth and fifth magnitude; brighter than $\varkappa$ and δ, and rather brighter than ζ Lyræ; good obſervation.

At $9\frac{1}{2}$ h. I thought it was decreaſed, being equal to ζ and rather brighter than $\varkappa$ Lyræ. Mr. E. PIGOTT alſo thought it was decreaſing.

Oct. 19. At $6\frac{1}{2}$ h. it was rather leſs than ζ and $\varkappa$, and brighter than δ Lyræ.

At 8¼ h. nearly the ſame, if not increaſed.

Oct. 20. At 6½ h. rather brighter than ξ and θ Herculis, and between the fourth and third magnitude.

At 8½ h. and 11 h. I thought it was increaſed, but it was leſs than γ Lyræ; between the third and fourth magnitude.

Oct. 22. At 6 h. 8 h. and 9 h. nearly equal to γ Lyræ.

Oct. 23. At 6 h. 8 h. and 11 h. rather leſs than γ, though nearly equal to it.

Oct. 24. At 6½ h. and 11 h. leſs than γ Lyræ, and brighter than ξ and θ Herculis; at 8 h. Mr. E. PIGOTT thought it rather leſs than γ Lyræ.

Oct. 25. At 6 h. 8 h. and 11 h. nearly, though perhaps not quite equal to θ Herculis; leſs than ξ Herculis, and brighter than ζ and δ Lyræ; about the fourth magnitude. At 6½ h. Mr. E. PIGOTT thought it rather brighter than θ and ο Herculis.

Oct. 26. At 6 h. and 11 h. brighter than θ and ξ Herculis, but leſs than γ Lyræ.

Oct. 27. At 6 h. and 8½ h. brighter than it was laſt night, but ſtill leſs than γ Lyræ; much brighter than ξ and θ Herculis; the moon was at its full.

Oct. 28. At 8 h. ± rather leſs than γ Lyræ.

Oct. 29. At 9½ h. nearly equal to, though rather brighter than γ Lyræ; I ſaw them but for a ſhort time on account of clouds coming on.

Oct. 31. At 8 h. between the fifth and fourth magnitude; leſs than ζ and ϰ, and brighter than δ Lyræ. Mr. E. PIGOTT thought it equal to ζ Lyræ at 8¼ h.

Nov. 1. At 6½ h. between the fourth and fifth magnitude; rather brighter than ζ, and brighter than ϰ and δ Lyræ.

Nov. 3. At 5½ h. little leſs than γ Lyræ.

Nov. 6. At 8 h. rather leſs than γ Lyræ, and brighter than θ Herculis. Mr. E. PIGOTT thought it nearly equal to γ Lyræ.

Nov. 7. At 7 h. and 10½ h. much leſs than γ Lyræ; nearly equal to, if not rather brighter than, θ Herculis, and rather leſs than ξ Herculis; between the fourth and third magnitude.

Nov. 10. At 10¾ h. nearly equal to γ Lyræ. Mr. E. PIGOTT thought it not quite ſo bright as γ at 11 h.

Nov. 11. At 5½ h. and 7 h. a little brighter than γ Lyræ; afterwards I rather thought them equal, though β appeared for the moſt part ſomething brighter. At 11 h. and 12 h. they appeared nearly equal. At 7 h. Mr. E. PIGOTT thought it was leſs than γ, if there was any difference.

Nov. 12. At 6½ h. 8½ h. and 10 h. much leſs than γ Lyræ, but brighter than ξ and θ Herculis; between the fourth and third magnitude.

Nov. 13. At 6½ h. and 10 h. equal to, if not rather leſs than ζ, leſs than $\varkappa$, and brighter than δ Lyræ; between the fifth and fourth magnitude. At 5½ h. Mr. E. PIGOTT thought it rather brighter than ζ Lyræ.

Nov. 16. At 7¼ h. little leſs than γ. At 10 h. certainly a little brighter than it.

Nov. 17. At 6 h. rather brighter than γ. At 8½ h. 9½ h. and 10½ h. brighter than γ, and leſs than β Cygni.

Nov. 18. At 9 h. 10 h. and 19 h. juſt the ſame.

Nov. 19. At 6½ h. and 8 h. leſs than γ Lyræ, and brighter than θ and ξ Herculis; between the third and fourth magnitude. At 10 h. ſomething brighter than θ Herculis.

Nov. 20. At 7 h. 8 h. and 10½ h. rather leſs than ξ, and rather brighter than θ Herculis; between the fourth and third

magnitude. At 18¼ h. I thought it was increaſed; obſerved in twilight.

Nov. 21. At 7 h. ſomething brighter than θ and ξ Herculis.

Nov. 25. At 7 h. leſs than γ Lyræ, and brighter than θ Herculis; between the fourth and third magnitude. At 9¼ h. I thought it was decreaſed, being now of the fourth magnitude.

Nov. 26. At 9 h. ± much leſs than γ, and of between the fourth and fifth magnitude; but the weather was too hazy, and the moon-light too ſtrong, to obſerve well.

Nov. 29. At 7½ h. and 8 h. rather brighter than γ Lyræ. Mr. EDW. PIGOTT thought it nearly equal to γ at 8 h.

Nov. 30. At 8¼ h. and 10¼ h. brighter than γ Lyræ, and leſs than β Cygni; air clear.

Dec. 4. At 5½ h. 6½ h. and 10½ h. leſs than γ Lyræ, and brighter than θ Herculis; between the third and fourth magnitude. Mr. E. PIGOTT thought it nearly equal to γ at 6½ h.

Dec. 9. At 8 h. much leſs than γ Lyræ, and brighter than ζ Lyræ; about between the fourth and fifth magnitude. At 18½ h. it was increaſed, and nearly equal to *o* Herculis; but leſs than θ and ξ; not quite of the fourth magnitude.

Dec. 11. At 6 h. and 8 h. leſs than γ Lyræ, and brighter than θ and ξ Herculis. At 8½ h. 9½ h. and 18½ h. nearly equal to, though rather leſs than γ.

Dec. 12. At 5 h. and 6 h. nearly equal to γ, though rather leſs.

Dec. 13. At 5½ h. and 9¼ h. ſomething brighter than γ.

Dec. 14. At 7 h. and 8½ h. rather brighter than γ.

Dec. 17. At 5½ h. leſs than γ Lyræ, and brighter than θ and ξ Herculis. At 7¾ h. nearly equal to γ, though rather leſs.

Dec.

Dec. 19. At 9 h. I believe it was brighter than γ, but the weather was not very favourable.

At 19 h. little leſs than γ.

Dec. 20. At $5\frac{1}{4}$ h. leſs than γ Lyræ, and brighter than θ and ξ Herculis. At $6\frac{1}{2}$ h. nearly equal, though rather leſs than γ Lyræ.

Dec. 21. At 8 h. much leſs than γ, and conſiderably brighter than ζ Lyræ; not quite of the fourth magnitude.

At 18 h. a little brighter than ζ and $\varkappa$, and brighter than δ Lyræ; between the fourth and fifth magnitude.

Dec. 28. At 6 h. leſs than γ and brighter than θ Herculis; between the third and fourth magnitude. At 8 h. nearly equal to θ Herculis; between the fourth and third magnitude.

1785, Jan. 5. At $5\frac{3}{4}$ h. about equal to θ Herculis; fourth magnitude.

Jan. 6. At $5\frac{3}{4}$ h. between γ Lyræ and θ Herculis, but rather nearer γ. At $8\frac{1}{2}$ h. it ſeemed a little brighter than γ.

From the above ſeries of obſervations I have deduced all the concluſions relative to the eight points of the variation, as they are ſtated in the beginning of this paper. However, as at firſt it may not clearly appear, that the ſtar has a more conſiderable diminution in the third point than in the ſeventh, it will not be improper to add a few words relating to that circumſtance: for proof of it, therefore, I refer to an attentive compariſon of the obſervations of Sept. 10. Sept. 23. Oct. 5 and 6. Oct. 18 and 19. &c. correſponding to the third point of the variation with thoſe of Sept. 29 and 30. Oct. 25. Nov. 7 and 19, &c. correſponding to the ſeventh point of the variation. It may be objected, that in ſome of the obſervations of the ſeventh point, the ſtar might have become ſtill more diminiſhed in the intermediate hours; but this is not probable,

becauſe in that point the ſtar has been obſerved of about the fourth magnitude at intervals much ſhorter than in the third point, ſo that, if it had continued to diminiſh, its diminution would have proceeded at a more rapid rate, which ſtill ſhews that there is at leaſt a difference between theſe two points.

With regard to the period of the variation, it is evident from a collation of the preceding obſervations in a coarſe way, that it is nearly twelve days and three quarters. To determine it with greater accuracy is a ſubject of conſiderable difficulty, in the preſent caſe; for unleſs we can obtain very exact points of compariſon, the period would come out erroneous, eſpecially if deduced from intervals conſiſting of only a very few periods, as is the caſe here. However, as I have been able to obtain a few obſervations of the middle of its obſcuration in the third point accurate enough for our purpoſe, I have formed the following calculation.

Times of the middle of its obſcuration in the third point.

		h.		d.	h.
1784, Oct.	6	1	} only a ſingle period of	12	21
—	18	22			
Oct.	18	22	} D° —	12	17
—	31	15			
Oct.	6	1	} two periods, each of	12	19
—	31	15			
			Hence the period on a mean is	12	19±

In aſcertaining the above times, I attended particularly to the neareſt obſervations both preceding and following. In the manner above ſtated the period may alſo be deduced from the middle of its obſcuration in the ſeventh point; but as theſe obſervations are not ſo exact as the above, I ſhall only, as a further

further confirmation, compare two of the moſt diſtant of them, *viz.* Sept. 29. 22 h. and Nov. 20. 6 h. which interval I find contains ſix periods, each of 12 d. 20 h. ±.

I have it in my intention to purſue the ſubject further, and when I have got a ſufficient number of obſervations, it will be eaſy to determine the period with greater exactneſs, and alſo at the ſame time to aſcertain the other particulars of the variation with more preciſion. In the mean while I wiſh that this account may be conſidered as being yet imperfect; but I was induced to ſend it in its preſent ſtate, in hopes that other aſtronomers may contribute by their obſervations to the elucidation of this phænomenon.

As β Lyræ is a quadruple ſtar, N° 3. of Mr. HERSCHEL's Vth Claſs of Double Stars *, I was deſirous to ſee if any of the ſmall ſtars near it would be affected by its different changes; but they ſeemed not to ſuffer any alteration, either when it was at its greateſt or at its leaſt brightneſs. I attended to this the more particularly becauſe the loſs of the ſtar's light was very conſiderable, and the phænomenon ſeemed to be occaſioned by a rotation on the ſtar's axis, under a ſuppoſition that there are ſeveral large dark ſpots upon its body, and that its axis is inclined to the earth's orbit.

I muſt not omit mentioning here that Mr. HERSCHEL, amongſt thoſe ſtars which he ſuppoſes to have undergone an alteration, reckons β or γ Lyræ; becauſe he obſerved that γ was much larger than β, while FLAMSTEED marks both of the ſame magnitude †. It may alſo be added, as ſhewing that β Lyræ varied in former times, that HEVELIUS, in his Catalogue, differs from FLAMSTEED, and marks γ of the third magnitude,

* Phil. Tranſ. for 1782, p. 142.

† Phil. Tranſ. for 1783, p. 256.

and β of between the fourth and third. I have, however, ſome doubts whether the variation of this ſtar does not entirely ceaſe or become leſs viſible in certain years. Theſe doubts ariſe from ſome obſervations of CASSINI in Phil. Tranſ. N° 73. p. 2198. where I find that in obſerving the new ſtar, which then appeared near the beak of the Swan, he compared it very frequently for upwards of a month to β and γ Lyræ, yet without perceiving, or even ſuſpecting, that β was variable, though it was eaſy for him to have perceived it, if the variation had then been even leſs than it is now.

I am, &c.

JOHN GOODRICKE.

X. *On the Motion of Bodies affected by Friction. By the Rev.* Samuel Vince, *A. M. of* Cambridge; *communicated by* Anthony Shepherd, *D. D. F. R. S. Plumian Profeſſor of Aſtronomy and experimental Philoſophy at* Cambridge.

Read November 25, 1784.

THE ſubject of the paper which I have now the honour of preſenting to the Royal Society, ſeems to be of a very conſiderable importance both to the practical mechanic and to the ſpeculative philoſopher; to the former, as a knowledge of the laws and quantity of the friction of bodies in motion upon each other will enable him at firſt to render his machines more perfect, and ſave him in a great meaſure the trouble of correcting them by trials; and to the latter, as thoſe laws will furniſh him with principles for his theory, which when eſtabliſhed by experiments will render his concluſions applicable to the real motion of bodies upon each other. But, however important a part of mechanics this ſubject may conſtitute, and however, from its obvious uſes, it might have been expected to have claimed a very conſiderable attention both from the mechanic and philoſopher, yet it has, of all the other parts of this branch of natural philoſophy, been the moſt neglected. The law by which the motions of bodies are retarded by friction has never, that I know of, been truly eſtabliſhed. MUSSCHENBROEK ſays, that in ſmall velocities the friction varies very nearly as the velocity, but that in great velocities the friction increaſes; he has alſo attempted to prove, that by increaſing

the weight of a body the friction does not always increaſe exactly in the ſame ratio; and that the ſame body, if by changing its poſition you change the magnitude of the ſurface on which it moves, will have its quantity of friction alſo changed. HELSHAM and FERGUSON, from the ſame kind of experiments, have endeavoured to prove, that the friction does *not* vary by changing the quantity of ſurface on which the body moves; and the latter of theſe aſſerts, that the friction increaſes very nearly as the velocity; and that by increaſing the weight, the friction is increaſed in the *ſame* ratio. Theſe different concluſions induced me to repeat their experiments, in order to ſee how far they were concluſive in reſpect to the principles deduced from them: when it appeared, that there was another cauſe operating beſides friction, which they had not attended to, and which rendered all their deductions totally inconcluſive. Of thoſe who have written on the theory, no one has eſtabliſhed it altogether on true principles: EULER (whoſe theory is extremely elegant, and which, as he has ſo fully conſidered the ſubject, would have precluded the neceſſity of offering any thing further, had its principles been founded on experiments) ſuppoſes the friction to vary in proportion to the velocity of the body, and its preſſure upon the plane, neither of which are true: and others, who have imagined that friction is a uniformly retarding force (and which conjecture will be confirmed by our experiments), have ſtill retained the other ſuppoſition, and therefore rendered their ſolutions not at all applicable to the caſes for which they were intended. I therefore endeavoured by a ſet of experiments to determine,

1ſt, *Whether friction be a uniformly retarding force.*

2dly, *The quantity of friction.*

3dly,

3dly, *Whether the friction varies in proportion to the preſſure or weight.*

4thly, *Whether the friction be the ſame on whichever of its ſurfaces a body moves.*

The experiments, in which I was aſſiſted by my ingenious friend the Rev. Mr. JONES, Fellow of Trinity College, were made with the utmoſt care and attention, and the ſeveral reſults agreed ſo very exactly with each other, that I do not ſcruple to pronounce them to be concluſive.

2. A plane was adjuſted parallel to the horizon, at the extremity of which was placed a pulley, which could be elevated or depreſſed in order to render the ſtring which connected the body and the moving force parallel to the plane. A ſcale accurately divided was placed by the ſide of the pulley perpendicular to the horizon, by the ſide of which the moving force deſcended; upon the ſcale was placed a moveable ſtage, which could be adjuſted to the ſpace through which the moving force deſcended in any given time, which time was meaſured by a well regulated pendulum clock vibrating ſeconds. Every thing being thus prepared, the following experiments were made to aſcertain the law of friction. But let me firſt obſerve, that if friction be a uniform force, the difference between it and the given force of the moving power muſt be alſo uniform, and therefore the moving body muſt deſcend with a uniformly accelerated velocity, and conſequently the ſpaces deſcribed from the beginning of the motion muſt be as the ſquares of the times, juſt as when there was no friction, only they will be diminiſhed on account of the friction.

3. EXP. 1. A body was placed upon the horizontal plane, and a moving force applied, which from repeated trials was found to deſcend $52\frac{1}{2}$ inches in 4″, for by the beat of the clock and

the ſound of the moving force when it arrived at the ſtage, the ſpace could be very accurately adjuſted to the time; the ſtage was then removed to that point to which the moving force would deſcend in 3″, upon ſuppoſition that the ſpaces deſcribed by the moving power were as the ſquares of the times; and the ſpace was found to agree very accurately with the time; the ſtage was then removed to that point to which the moving force ought to deſcend in 2″, upon the ſame ſuppoſition, and the deſcent was found to agree exactly with the time; laſtly, the ſtage was adjuſted to that point to which the moving force ought to deſcend in 1″, upon the ſame ſuppoſition, and the ſpace was obſerved to agree with the time. Now, in order to find whether a difference in the time of deſcent could be obſerved, by removing the ſtage a little above and below the poſitions which correſponded to the above times, the experiment was tried, and the deſcent was always found too ſoon in the former, and too late in the latter caſe; by which I was aſſured that the ſpaces firſt mentioned correſponded exactly to the times. And, for the greater certainty, each deſcent was repeated eight or ten times; and every caution uſed in this experiment was alſo made uſe of in all the following.

EXP. 2. A ſecond body was laid upon the horizontal plane, and a moving force applied which deſcended 41¾ inches in 3″; the ſtage was then adjuſted to the ſpace correſponding to 2″, upon ſuppoſition that the ſpaces deſcended through were as the ſquares of the times, and it was found to agree accurately with the time; the ſtage was then adjuſted to the ſpace correſponding to 1″, upon the ſame ſuppoſition, and it was found to agree with the time.

EXP. 3. A third body was laid upon the horizontal plane, and a moving force applied, which deſcended 59⅝ inches in 4″; the

ſtage

ſtage was then adjuſted to the ſpace correſponding to 3″, upon ſuppoſition that the ſpaces deſcended through were as the ſquares of the times, and it was found to agree with the time; the ſtage was then adjuſted to the ſpace correſponding to 2″, upon the ſame ſuppoſition, and it was found to agree with the time; the ſtage was then adjuſted to the ſpace correſponding to 1″, and was found to agree with the time.

Exp. 4. A fourth body was then taken and laid upon the horizontal plane, and a moving force applied, which deſcended 55 inches in 4″; the ſtage was then adjuſted to the ſpace through which it ought to deſcend in 3″, upon ſuppoſition that the ſpaces deſcended through were as the ſquares of the times, and it was found to agree with the time; the ſtage was then adjuſted to the ſpace correſponding to 2″, upon the ſame ſuppoſition, and was found to agree with the time; laſtly, the ſtage was adjuſted to the ſpace correſponding to 1″, and it was found to agree exactly with the time.

Beſides theſe experiments, a great number of others were made with hard bodies, or thoſe whoſe parts ſo firmly cohered as not to be moved *inter ſe* by the friction; and in each experiment bodies of very different degrees of friction were choſen, and the reſults all agreed with thoſe related above; we may therefore conclude, that *the friction of hard bodies in motion is a uniformly retarding force.*

But to determine whether the ſame was true for bodies when covered with cloth, woollen, &c. experiments were made in order to aſcertain it; when it was found in all caſes, that the retarding force increaſed with the velocity; but, upon covering bodies with paper, the conſequences were found to agree with thoſe related above.

4. Having proved that the retarding force of all hard bodies arising from friction is uniform, the quantity of friction, considered as equivalent to a weight without inertia drawing the body on the horizontal plane backwards, or acting contrary to the moving force, may be immediately deduced from the foregoing experiments. For let M = the moving force expressed by its weight; F = the friction; W = the weight of the body upon the horizontal plane; S = the space through which the moving force descended in the time t expressed in seconds; $r = 16\frac{1}{12}$ feet; then the whole accelerative force (the force of gravity being unity) will be $\frac{M-F}{M+W}$; hence, by the laws of uniformly accelerated motions, $\frac{M-F}{M+W} \times rt^2 = S$, consequently $F = M - \frac{\overline{M+W} \times S}{rt^2}$. To exemplify this, let us take the case of the last experiment, where M = 7, $W = 25\frac{3}{4}$, $S = 4\frac{7}{12}$ feet, $t = 4''$; hence $F = 7 - \frac{32\frac{3}{4} \times 4\frac{7}{12}}{16\frac{1}{12} \times 16} = 6.417$; consequently the friction was to the weight of the rubbing body as 6.4167 to 25.75. And the great accuracy of determining the friction by this method is manifest from hence, that if an error of 1 inch had been made in the descent (and experiments carefully made may always determine the space to a much greater exactness) it would not have affected the conclusion $\frac{1}{600}$dth part of the whole.

5. We come in the next place to determine, whether friction, *cæteris paribus*, varies in proportion to the weight or pressure. Now if the whole quantity of the friction of a body, measured by a weight without inertia equivalent to the friction drawing the body backwards, increases in proportion to its weight, it is manifest, that the retardation of the velocity of the body arising from the friction will not be altered; for the

retardation varies as $\frac{\text{Quantity of friction}}{\text{Quantity of matter}}$; hence, if a body be put in motion upon the horizontal plane by any moving force, if both the weight of the body and the moving force be increaſed in the ſame ratio, the acceleration ariſing from that moving force will remain the ſame, becauſe the accelerative force varies as the moving force divided by the whole quantity of matter, and both are increaſed in the ſame ratio; and if the quantity of friction increaſes alſo as the weight, then the retardation ariſing from the friction will, from what has been ſaid, remain the ſame, and therefore the whole acceleration of the body will not be altered; conſequently the body ought, upon this ſuppoſition, ſtill to deſcribe the ſame ſpace in the ſame time. Hence, by obſerving the ſpaces deſcribed in the ſame time, when both the body and the moving force are increaſed in the ſame ratio, we may determine whether the friction increaſes in proportion to the weight. The following experiments were therefore made in order to aſcertain this matter.

Exp. 1. A body weighing 10 oz. by a moving force of 4 oz. deſcribed in 2″ a ſpace of 51 inches; by loading the body with 10 oz. and the moving force with 4 oz. it deſcribed 56 inches in 2″; and by loading the body again with 10 oz. and the moving force with 4 oz. it deſcribed 63 inches in 2″.

Exp. 2. A body, whoſe weight was 16 oz. by a moving force of 5 oz. deſcribed a ſpace of 49 inches in 3″; and by loading the body with 64 oz. and the moving force with 20 oz. the ſpace deſcribed in the ſame time was 64 inches.

Exp. 3. A body weighing 6 oz. by a moving force of $2\frac{1}{2}$ oz. deſcribed 28 inches in 2″; and by loading the body with 24 oz. and the moving force with 10 oz. the ſpace deſcribed in the ſame time was 54 inches.

EXP. 4. A body weighing 8 oz. by a moving force of 4 oz. deſcribed $33\frac{1}{2}$ inches in 2″; and by loading the body with 8 oz. and the moving force with 4 oz. the ſpace deſcribed in the ſame time was 47 inches.

EXP. 5. A body whoſe weight was 9 oz. by a moving force of $4\frac{1}{2}$ oz. deſcribed 48 inches in 2″; and by loading the body with 9 oz. and the moving force with $4\frac{1}{2}$ oz. the ſpace deſcribed in the ſame time was 60 inches.

EXP. 6. A body weighing 10 oz. by a moving force of 3 oz. deſcribed 20 inches in 2″; by loading the body with 10 oz. and the moving force with 3 oz. the ſpace deſcribed in the ſame time was 31 inches; and by loading the body again with 30 oz. and the moving force with 9 oz. the ſpace deſcribed was 34 inches in 2″.

From theſe experiments, and many others which it is not neceſſary here to relate, it appears, that the ſpace deſcribed is always increaſed by increaſing the weight of the body and the accelerative force in the ſame ratio; and as the acceleration ariſing from the moving force continued the ſame, it is manifeſt, that the retardation ariſing from the friction muſt have been diminiſhed, for the whole accelerative force muſt have been increaſed on account of the increaſe of the ſpace deſcribed in the ſame time; and hence (as the retardation from friction varies as $\left(\frac{\text{Quantity of friction}}{\text{Quantity of matter}}\right)$ *the quantity of friction increaſes in a leſs ratio than the quantity of matter or weight of the body.*

6. We come now to the laſt thing which it was propoſed to determine, that is, whether the friction varies by varying the ſurface on which the body moves. Let us call two of the ſurfaces A and *a*, the former being the greater, and the latter the leſs. Now the weight on every given part of *a* is as much greater

than the weight on an equal part of A, as A is greater than *a*; if therefore the friction was in proportion to the weight, *cæteris paribus*, it is manifeſt, that the friction on *a* would be equal to the friction on A, the whole friction being, upon ſuch a ſuppoſition, as the weight on any given part of each ſurface multiplied into the number of ſuch parts or into the whole area, which products, from the proportion above, are equal. But from the laſt experiments it has been proved, that the friction on any given ſurface increaſes in a leſs ratio than the weight; conſequently the friction on any given part of *a* has a leſs ratio to the friction on an equal part of A than A has to *a*, and hence the friction on *a* is leſs than the friction on A, that is, the ſmalleſt ſurface has always the leaſt friction. But as this concluſion is contrary to the generally received opinion, I have thought it proper to confirm the ſame by a ſet of experiments. But before I proceed to relate them, I will beg leave to recommend to thoſe, who may afterwards be induced to repeat them, the following cautions, which are extremely neceſſary to be attended to. Great care muſt be taken that the two ſurfaces have exactly the ſame degree of roughneſs; in order to be certain of which, ſuch bodies muſt be choſen as have no knots in them, and whoſe grain is ſo very regular that when the two ſurfaces are planed with a fine rough plane, their roughneſs may be the ſame, which will not be the caſe if the body be knotty, or the grain irregular, or if it happens not to run in the ſame direction on both ſurfaces. When you cannot depend on the ſurfaces having the ſame degree of roughneſs, the beſt way will be to paſte ſome fine rough paper on each ſurface, which perhaps will give a more equal degree of roughneſs than can be obtained by any other method. Now as the proof which I have already given depends only on the motion

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of the body upon the *ſame* ſurface, it is not liable to any inaccuracy of the kind which the preceding cautions have been given to avoid, nor indeed to any other, and therefore it muſt be perfectly concluſive. In the following experiments the cautions mentioned above were carefully attended to.

EXP. 1. A body was taken whoſe flat ſurface was to its edge as 22 : 9, and with the ſame moving force the body deſcribed on its flat ſide $33\frac{1}{2}$ inches in 2″, and on its edge 47 inches in the ſame time.

EXP. 2. A ſecond body was taken whoſe flat ſurface was to its edge as 32 : 3, and with the ſame moving force it deſcribed on its flat ſide 32 inches in 2″, and on its edge it deſcribed $37\frac{1}{2}$ inches in the ſame time.

EXP. 3. I took another body and covered one of its ſurfaces, whoſe length was 9 inches, with a fine rough paper, and by applying a moving force, it deſcribed 25 inches in 2″; I then took off ſome paper from the middle, leaving only $\frac{3}{8}$ of an inch at the two ends, and with the ſame moving force it deſcribed 40 inches in the ſame time.

EXP. 4. Another body was taken which had one of its ſurfaces, whoſe length was 9 inches, covered with a fine rough paper, and by applying a moving force it deſcribed 42 inches in 2″; ſome of the paper was then taken off from the middle, leaving only $1\frac{3}{8}$ inches at the two ends, and with the ſame moving force it deſcribed 54 inches in 2″; I then took off more paper, leaving only $\frac{1}{4}$ of an inch at the two ends, and the body then deſcribed, by the ſame moving force, 60 inches in the ſame time.

In the two laſt experiments the paper which was taken off the ſurface was laid on the body, that its weight might not be altered.

EXP.

Exp. 5. A body was taken whose flat surface was to its edge as 30 : 17; the *flat* side was laid upon the horizontal plane, a moving force was applied, and the stage was fixed in order to stop the moving force, in consequence of which the body would then go on with the velocity acquired until the friction had destroyed all its motion; when it appeared from a mean of 12 trials that the body moved, after its acceleration ceased, $5\frac{2}{3}$ inches before it stopped. The *edge* was then applied, and the moving force descended through the same space, and it was found, from a mean of the same number of trials, that the space described was $7\frac{1}{3}$ inches before the body lost all its motion, after it ceased to be accelerated.

Exp. 6. Another body was then taken whose flat surface was to its edge as 60 : 19, and, by proceeding as before, on the flat surface it described, at a mean of 12 trials, $5\frac{1}{4}$ inches, and on the edge $6\frac{1}{4}\frac{1}{4}$ inches, before it stopped, after the acceleration ceased.

Exp. 7. Another body was taken whose flat surface was to its edge as 26 : 3, and the spaces described on these two surfaces, after the acceleration ended, were, at a mean of 10 trials, $4\frac{3}{7}$ and $7\frac{7}{10}$ inches respectively.

From all these different experiments it appears, that the smallest surface had always the least friction, which agrees with the consequence deduced from the consideration that the friction does not increase in so great a ratio as the weight; we may therefore conclude, that *the friction of a body does not continue the same when it has different surfaces applied to the plane on which it moves, but that the smallest surface will have the least friction.*

7. Having thus established, from the most decisive experiments, all that I proposed relative to friction, I think it proper,

before

before I conclude, to give the result of my examination into the nature of the experiments which have been made by others; which were repeated, in order to see how far they were conclusive in respect to the principles which have been deduced from them. The experiments which have been made by all the authors that I have seen, have been thus instituted. To find what moving force would *just* put a body at rest in motion: and they concluded from thence, that the accelerative force was then equal to the friction; but it is manifest, that any force which will put a body in motion must be *greater* than the force which opposes its motion, otherwise it could not overcome it; and hence, if there were no other objection than this, it is evident, that the friction could not be very accurately obtained; but there is another objection which totally destroys the experiment so far as it tends to show the quantity of friction, which is the strong cohesion of the body to the plane when it lies at rest; and this is confirmed by the following experiments. 1st, A body of $12\frac{3}{4}$ oz. was laid upon an horizontal plane, and then loaded with a weight of 8 lb. and such a moving force was applied as would, when the body was just *put* in motion, continue that motion without any acceleration, in which case the friction must be just equal to the accelerative force. The body was then stopped, when it appeared, that the same moving force which had *kept* the body in motion before, would not *put* it in motion, and it was found necessary to take off $4\frac{1}{2}$ oz. from the body before the same moving force *would* put it in motion; it appears, therefore, that this body, when laid upon the plane at rest, acquired a very strong cohesion to it. 2dly, A body whose weight was 16 oz. was laid at rest upon the horizontal plane, and it was found that a moving force of 6 oz. would just *put* it in motion; but that a moving force of 4 oz.

would

would, when it was juſt put in motion, *continue* that motion without any acceleration, and therefore the accelerative force muſt *then* have been equal to the friction, and not when the moving force of 6 oz. was applied.

From theſe experiments therefore it appears, how very conſiderable the coheſion was in proportion to the friction when the body was in motion; it being, in the latter caſe, almoſt $\frac{1}{3}$, and in the former it was found to be very nearly equal to the whole friction. All the concluſions therefore deduced from the experiments, which have been inſtituted to determine the friction from the force neceſſary to *put* a body in motion (and I have never ſeen any deſcribed but upon ſuch a principle) have manifeſtly been totally falſe; as ſuch experiments only ſhew the reſiſtance which ariſes from the coheſion and friction conjointly.

8. I ſhall conclude this part of the ſubject with a remark upon Art. 5. It appears from all the experiments which I have made, that the proportion of the increaſe of the friction to the increaſe of the weight was different in all the different bodies which were made uſe of; no general rule therefore can be eſtabliſhed to determine this for *all* bodies, and the experiments which I have hitherto made have not been ſufficient to determine it for the *ſame* body. At ſome future opportunity, when I have more leiſure, I intend to repeat the experiments in order to eſtabliſh, in ſome particular caſes, the law by which the quantity of friction increaſes by increaſing the weight. Leaving this ſubject therefore for the preſent, I ſhall proceed to eſtabliſh a theory upon the principles which we have already deduced from our experiments.

PROPOSITION I.

Let e, f, g, (fig. 1.) *repreſent either a cylinder, or that circular ſection of a body on which it rolls down the inclined plane* CA *in conſequence of its friction, to find the time of deſcent and the number of revolutions.*

As it has been proved in Art. 5. that the friction of a body does not increaſe in proportion to its weight or preſſure, we cannot therefore, by knowing the friction on any other plane, determine the friction on CA; the friction therefore on CA can only be determined by experiments made upon *that* plane, that is, by letting the body deſcend from reſt, and obſerving the ſpace deſcribed in the firſt ſecond of time; call that ſpace a, and then, as by Art. 3. friction is a uniformly retarding force, the body muſt be uniformly accelerated, and conſequently the whole time of deſcent in ſeconds will be $=\sqrt{\frac{AC}{a}}$. Now to determine the number of revolutions, let s be the center of oſcillation to the point of ſuſpenſion a *; then, becauſe no force acting at a can affect the motion of the point s, that point, notwithſtanding the action of the friction at a, will always have a motion parallel to CA uniformly accelerated by a force equal to that with which the body would be accelerated if it had no friction; hence, if $2m = 32\frac{1}{6}$ feet, the velocity acquired by the point s in the firſt ſecond will be $=\frac{2m \times CB}{CA}$; now the exceſs of the ve-

* a and s are not fixed points in the body, but the former always repreſents that point of the body in contact with the plane, and the latter the correſponding center of oſcillation.

locity of the point s above that of r (r being the center) is manifeſtly the velocity with which s is carried about r; hence the velocity of s about the center $= \frac{2m \times CB}{CA} - 2a = \frac{2m \times CB - 2a \times CA}{CA}$, conſequently $rs : ra :: \frac{2m \times CB - 2a \times CA}{CA} : \frac{2m \times ra \times CB - 2a \times ra \times CA}{rs \times CA}$ = the velocity with which a point of the circumference is carried about the center, and which therefore expreſſes the force which accelerates the rotation; now as $2a$ expreſſes the accelerative force of the body down the plane, and the ſpaces deſcribed in the ſame time are in proportion to thoſe forces, we have $2a : CA :: \frac{2m \times ra \times CB - 2a \times ra \times CA}{rs \times CA} : \frac{m \times ra \times CB - a \times ra \times CA}{a \times rs}$ the ſpace which any point of the circumference deſcribes about the center in the whole time of the body's deſcent down CA; which being divided by the circumference $p \times ra$ (where $p = 6.283$ &c.) will give $\frac{m \times BC - a \times AC}{p \times a \times rs}$ for the whole number of revolutions required.

Cor. 1. If $a \times CA = m \times BC$, the number of revolutions $= 0$, and therefore the body will then only ſlide; conſequently the friction vaniſhes.

Cor. 2. Let $a'r's'$ (fig. 2.) be the next poſition of ars, and draw $tr'b$ parallel to sa, then will $s't$ repreſent the retardation of the center r ariſing from friction, and $a'b$ will repreſent the acceleration of a point of the circumference about its center; hence the retardation of the center : acceleration of the circumference about the center :: $s't : a'b$ :: (by ſim. Δ's) $tr' : br' :: rs : ra$.

Cor. 3. If a' coincides with a, the body does not *ſlide* but only *roll*; now in this caſe $ss' : rr' :: as : ar$; but as ss' and rr' repreſent the ratio of the velocities of the points s and r,

they will be to each other as $\frac{2m \times BC}{CA} : 2a$ or as $m \times CB : a \times CA$; hence, when the body *rolls* without *ſliding*, $as : ar :: m \times CB : a \times CA$.

Cor. 4. The time of deſcent down CA is $= \sqrt{\frac{\overline{AC}}{a}}$; but by the laſt Cor. when the body *rolls* without *ſliding*, $a = \frac{m \times ra \times BC}{sa \times AC}$, hence the time of deſcent in that caſe $= AC \sqrt{\frac{sa}{m \times ra \times BC}}$; now the time of deſcent, if there were no friction, would be $= \frac{AC}{\sqrt{m \times BC}}$, hence the time of deſcent, when the body *rolls* without *ſliding* : time of free deſcent $:: \sqrt{sa} : \sqrt{ra}$.

Cor. 5. By the laſt Cor. it appears, that when the body juſt *rolls* without *ſliding*, or when the friction is juſt equal to the accelerative force, the time of deſcent $= AC \sqrt{\frac{sa}{m \times ra \times BC}}$; now it is manifeſt, that the time of deſcent will continue the ſame, if the friction be increaſed, for the body will ſtill freely roll, as no increaſe of the friction acting at a can affect the motion of the point s.

If the body be projected from C with a velocity, and at the ſame time have a rotatory motion, the time of deſcent and the number of revolutions may be determined from the common principles of uniformly accelerated motions, as we have already inveſtigated the accelerative force of the body down the plane and of its rotation about its axis; it ſeems therefore unneceſſary to lengthen out this paper with the inveſtigations.

PRO-

PROPOSITION II.

Let the body be projected on an horizontal plane LM (fig. 3.) *with a given velocity, to determine the space through which the body will move before it stops, or before its motion becomes uniform.*

CASE I. 1. Suppose the body to have no rotatory motion when it begins to move; and let a = the velocity of projection *per* second measured in feet, and let the retarding force of the friction of the body, measured by the velocity of the body which it can destroy in one second of time, be determined by experiment and called F, and let x be the space through which the body would move by the time its motion was all destroyed when projected with the velocity a, and retarded by a force F; then, from the principles of uniformly retarded motion, $x = \frac{a^2}{2F}$, and if t = time of describing that space, we have $t = \frac{a}{F}$, and hence the space described in the first second of time $= \frac{2a - F}{2}$. Now it is manifest, that when the rotatory motion of the body about its axis is equal to its progressive motion, the point a will be carried backwards by the *former* motion as much as it is carried forwards by the *latter*; consequently the point of contact of the body with the plane will then have no motion in the direction of the plane, and hence the friction will at that instant cease, and the body will continue to *roll* on uniformly without *sliding* with the velocity which it has at that point. Put therefore z = the space described from the commencement of the motion till it becomes uniform, then the body being uniformly retarded, the spaces from the end of

the motion vary as the ſquares of the velocities, hence $\frac{a^2}{2F} : a^2 \; (:: 1 : 2F) :: \frac{a^2}{2F} - z : a^2 - 2Fz =$ ſquare of the progreſſive velocity when the motion becomes uniform; therefore the velocity deſtroyed by friction $= a - \sqrt{a^2 - 2Fz}$; hence, as the velocity generated or deſtroyed in the ſame time is in proportion to the force, we have by Cor. 2. Prop. 1. $r s : r a :: a - \sqrt{a^2 - 2Fz} : \frac{ra}{rs} \times \overline{a - \sqrt{a^2 - 2Fz}}$ the velocity of the circumference $e f g$ generated about the center, conſequently $\sqrt{a^2 - 2Fz} = \frac{ra}{rs} \times \overline{a - \sqrt{a^2 - 2Fz}}$, and hence $z = \frac{\overline{rs^2 + 2rs \times ra} \times a^2}{as^2 \times 2F}$ the ſpace which the body deſcribes before the motion becomes uniform.

2. If we ſubſtitute this value of z into the expreſſion for the velocity, we ſhall have $a \times \frac{ra}{rs}$ for the velocity of the body when its motion becomes uniform; hence therefore it appears, that the velocity of the body, when the friction ceaſes, will be the ſame whatever be the quantity of the friction. If the body be the circumference of a circle, it will always loſe half the velocity before its motion becomes uniform.

CASE II. 1. Let the body, beſides having a progreſſive velocity in the direction LM (fig. 3.) have alſo a rotatory motion about its center in the direction $g f e$, and let v repreſent the initial velocity of any point of the circumference about the center, and ſuppoſe it firſt to be leſs than a; then friction being a uniformly retarding force, no alteration of the velocity of the point of contact of the body upon the plane can affect the quantity of friction; hence the progreſſive velocity of the body will be the ſame as before, and conſequently the rotatory velo-

city

city generated by friction will alſo be the ſame, to which if we add the velocity about the center at the beginning of the motion, we ſhall have the whole rotatory motion; hence therefore, $v+\frac{ra}{rs}\times\overline{a-\sqrt{a^2-2Fz}}=\sqrt{a^2-2Fz}$, conſequently $z=\frac{a^2\times as^2-\overline{v\times rs+a\times ra}^2}{2F\times as^2}$ the ſpace deſcribed before the motion becomes uniform.

2. If this value of z be ſubſtituted into the expreſſion for the velocity, we ſhall have $\frac{v\times rs+a\times ra}{as}$ for the velocity when the friction ceaſes.

3. If $v=a$, then $z=o$, and hence the body will continue to move uniformly with the firſt velocity.

4. If v be greater than a, then the rotatory motion of the point a on the plane being greater than its progreſſive motion and in a contrary direction, the abſolute motion of the point a upon the plane will be in the direction ML, and conſequently friction will now act in the direction LM in which the body moves, and therefore will accelerate the *progreſſive* and retard the *rotatory* motion; hence it appears, that *the progreſſive motion of a body may be* ACCELERATED *by friction.* Now to determine the ſpace deſcribed before the motion becomes uniform, we may obſerve, that as the progreſſive motion of the body is now accelerated, the velocity after it has deſcribed any ſpace z will be $=\sqrt{a^2+2Fz}$, hence the velocity acquired $=\sqrt{a^2+2Fz}-a$, and conſequently the rotatory velocity deſtroyed $\frac{ra}{rs}\times\overline{\sqrt{a^2+2Fz}-a}$, hence $v-\frac{ra}{rs}\times\overline{\sqrt{a^2+2Fz}-a}=\sqrt{a^2+2Fz}$, therefore $z=\frac{\overline{rs\times v+ra\times a}^2-a^2\times as^2}{2F\times as^2}$ the ſpace required.

5. If

5. If $a = o$, or the body be placed upon the plane without any progreſſive velocity, then $z = \frac{rs^2 \times v^2}{2F \times as}$.

CASE III. 1. Let the given rotatory motion be in the direction $g\,e\,f$; then as the friction muſt in this caſe always act in the direction ML, it muſt continually tend to deſtroy both the progreſſive and rotatory motion. Now as the velocity deſtroyed in the ſame time is in proportion to the retarding force, and the force which retards the *rotatory* is to the force which retards the *progreſſive* velocity by Cor. 2. Prop. 1. as $ra : rs$, therefore if v be to a as ra is to rs, then the retarding forces being in proportion to the velocities, both motions will be deſtroyed together, and conſequently the body, after deſcribing a certain ſpace, will reſt; which ſpace, being that deſcribed by the body uniformly retarded by the force F, will, from what was proved in Caſe I. be equal to $\frac{a^2}{2F}$.

2. If v bears a greater proportion to a than ra does to rs, it is manifeſt, that the rotatory motion will not be all deſtroyed when the progreſſive is; conſequently the body, after it has deſcribed the ſpace $\frac{a^2}{2F}$, will return back in the direction ML; for the progreſſive motion being then deſtroyed, and the rotatory motion ſtill continuing in the direction $g\,e\,f$, will cauſe the body to return with an accelerative velocity until the friction ceaſes by the body's beginning to roll, after which it will move on uniformly. Now to determine the ſpace deſcribed before this happens, we have $rs : ra :: a : \frac{ra \times a}{rs}$ the rotatory velocity deſtroyed when the progreſſive is all loſt; hence $v - \frac{ra \times a}{rs} = \frac{v \times rs - a \times ra}{rs}$ = the rotatory velocity at that time, which being

being ſubſtituted for v in the laſt article of Caſe II. gives $\frac{\overline{v+rs-a\times ra}^{2}}{2F\times as^{2}}$ for the ſpace deſcribed before the motion becomes uniform.

3. If v has a leſs proportion to a than ra has to rs, it is manifeſt, that the *rotatory* motion will be deſtroyed before the *progreſſive*; in which caſe a rotatory motion will be generated in a contrary direction until the two motions become equal, when the friction will inſtantly ceaſe, and the body will then move on uniformly. Now $ra : rs :: v : \frac{v\times rs}{ra}$ the progreſſive velocity deſtroyed when the rotatory velocity ceaſes, hence $a-\frac{v\times rs}{ra}=\frac{a\times ra-v\times rs}{ra}=$ progreſſive velocity when it begins its rotatory motion in a contrary direction; ſubſtitute therefore this quantity for a in the expreſſion for z in Caſe I. and we have $\frac{\overline{rs^{2}+2rs\times ra}\times\overline{a\times ra-v\times rs}^{2}}{as^{2}\times ar^{2}\times 2F}$ for the ſpace deſcribed after the rotatory motion ceaſes before the motion of the body becomes uniform. Now to determine the ſpace deſcribed before the rotatory motion was all deſtroyed, we have (as the ſpace from the end of a uniformly retarded motion varies as the ſquare of the velocity) $a^{2} : \frac{a^{2}}{2F} :: \overline{\frac{a\times ra-v\times rs}{ra^{2}}}^{2} : \overline{\frac{a\times ra-v\times rs}{2F\times ra^{2}}}^{2}$ the ſpace that could have been deſcribed from the time that the rotatory velocity was deſtroyed, until the progreſſive motion would have been deſtroyed had the fricton continued to act; hence $\frac{a^{2}}{2F}-\overline{\frac{a\times ra-v\times rs}{2F\times ra^{2}}}^{2}=\frac{2av\times ra\times rs-v^{2}\times rs^{2}}{2F\times ra^{2}}=$ the ſpace deſcribed when the rotatory motion was all deſtroyed, hence $\frac{\overline{rs^{2}+2rs\times ra}\times\overline{a\times ar-v\times sr}^{2}}{as^{2}\times ar^{2}\times 2F}+\frac{2av\times ra\times rs-v^{2}\times rs^{2}}{2F\times ra^{2}}=$ whole ſpace deſcribed by the body before its motion becomes uniform.

DEFINITION.

The CENTER *of* FRICTION *is that point in the baſe of a body on which it revolves, into which if the whole ſurface of the baſe, and the maſs of the body were collected, and made to revolve about the center of the baſe of the given body, the angular velocity deſtroyed by its friction would be equal to the angular velocity deſtroyed in the given body by its friction in the ſame time.*

PROPOSITION III.

To find the center of friction.

Let FGH (fig. 4.) be the baſe of a body revolving about its center C, and ſuppoſe about *a*, *b*, *c*, &c. to be indefinitely ſmall parts of the baſe, and let A, B, C, &c. be the correſponding parts of the ſolid, or the priſmatic parts having *a*, *b*, *c*, &c. for their baſes; and P the center of friction. Now it is manifeſt, that the decrement of the angular velocity muſt vary as the whole diminution of the momentum of rotation cauſed by the friction *directly*, and as the whole momentum of rotation or effect of the inertia of all the particles of the ſolid *inverſely*; the *former* being employed in diminiſhing the angular velocity, and the *latter* in oppoſing that diminution by the endeavour of the particles to perſevere in their motion. Hence, if the effect of the friction varies as the effect of the inertia, the decrements of the angular velocity in a given time will be equal. Now as the quantity of friction (as has been proved from experiments) does not depend on the velocity, the effect of the friction of the elementary parts of the baſe *a*, *b*, *c*, &c.

will be as $a \times aC$, $b \times bC$, $c \times cC$, &c. alſo the effect of the inertia of the correſponding parts of the body will be as $A \times aC^2$, $B \times bC^2$, $C \times cC^2$, &c. Now when the whole ſurface of the baſe and maſs of the body are concentrated in P, the effect of the friction will be as $\overline{a+b+c+}$ &c. $\times$ CP, and of the inertia as $\overline{A+B+C+}$ &c. $\times$ CP^2; conſequently $a \times aC + b \times bC + c \times cC$ + &c. : $\overline{a+b+c+}$ &c. $\times$ CP :: $A \times aC^2 + B \times bC^2 + C \times cC^2$ + &c. : $\overline{A+B+C+}$ &c. $\times$ CP^2; and hence

$$CP = \frac{\overline{A \times aC^2 + B \times bC^2 + C \times cC^2 +} \text{ &c.} \times \overline{a+b+c+\&}}{a \times aC + b \times bC + c \times cC + \text{&c.} \times \overline{A+B+C+\&c.}} = \text{(if S} =$$

the ſum of the products of each particle into the ſquare of its diſtance from the axis of motion, T = the ſum of the products of each part of the baſe into its diſtance from the center, s = the area of the baſe, t = the ſolid content of the body) $\frac{S \times s}{T \times t}$.

PROPOSITION IV.

Given the velocity with which a body begins to revolve about the center of its baſe, to determine the number of revolutions which the body will make before all its motion be deſtroyed.

Let the friction, expreſſed by the velocity which it is able to deſtroy in the body if it were projected in a right line horizontally in one ſecond, be determined by experiment, and called F; and ſuppoſe the initial velocity of the center of friction P about C to be a. Then conceiving the whole ſurface of the baſe and maſs of the body to be collected into the point P, and (as has been proved in Prop. II.) $\frac{a^2}{2F}$ will be the ſpace which the body ſo concentrated will deſcribe before all its motion be deſtroyed;

hence if we put $z = PC$, $p =$ the circumference of a circle whoſe radius is unity, then will $pz =$ circumference deſcribed by the point P; conſequently $\frac{a^2}{2pzF} =$ the number of revolutions required.

Cor. If the ſolid be a cylinder and r be the radius of its baſe, then $z = \frac{3r}{4}$, and therefore the number of revolutions $= \frac{2a^2}{3prF}$.

PROPOSITION V.

To find the nature of the curve deſcribed by any point of a body affected by friction, when it deſcends down any inclined plane.

Let efg (fig. 5.) be the body, the points a, r, s, as in Prop. I. and conceive st, rn, to be two indefinitely ſmall ſpaces deſcribed by the points s and r in the ſame time, and which therefore will repreſent the velocities of thoſe points; but from Prop. I. the ratio of theſe velocities is expreſſed by $m \times CB : a \times CA$, hence $st : rn :: m \times CB : a \times CA$. With the center r let a circle vw be deſcribed touching the plane LM which is parallel to AC at the point b, and let the radius of this circle be ſuch that, conceiving it to deſcend upon the plane LM along with the body deſcending on CA, the point b may be at reſt, or the circle may roll without ſliding. To determine which radius, produce rs to x, parallel to which draw ndy, and produce nt to z; now it is manifeſt, that in order to anſwer the conditions above-mentioned, the velocity of the point x muſt be to the velocity of the point r as 2 : 1, that is, $zx : yx :: 2 : 1$, hence $zy = yx = nr$. Now $zy : dt$ $(:: ny : nd) :: rx : rs$; therefore $dt = \frac{rs}{rx} \times zy = \frac{rs}{rx} \times nr$, hence ts $(= td + ds = td + nr =$

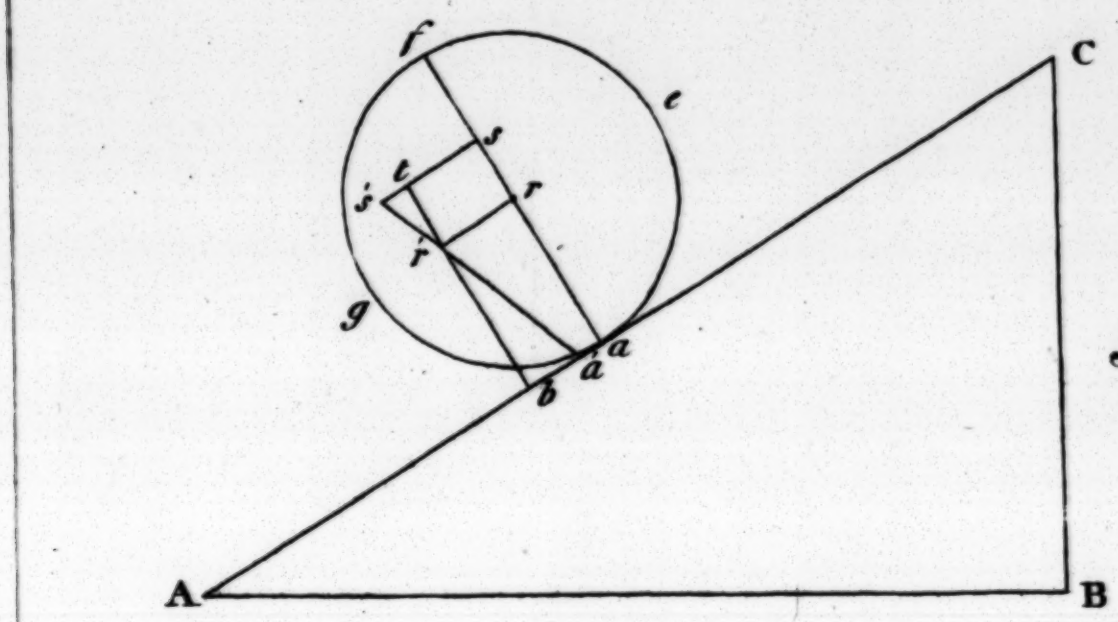

Fig. 2.

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Fig. 4.

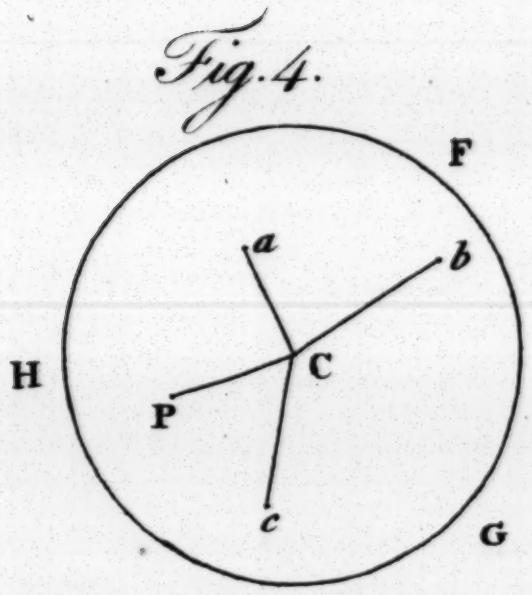

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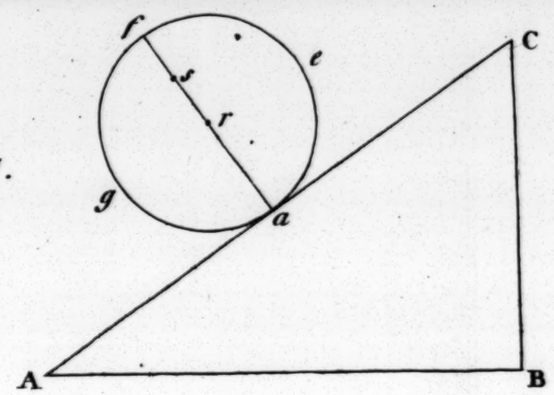
Fig. 1.



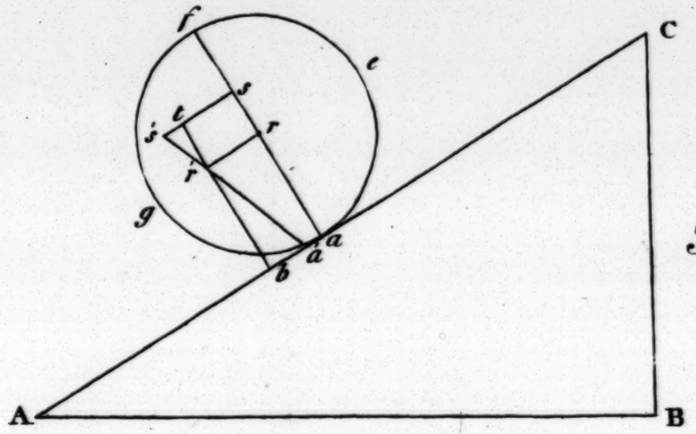


Fig. 2.

Fig. 3.

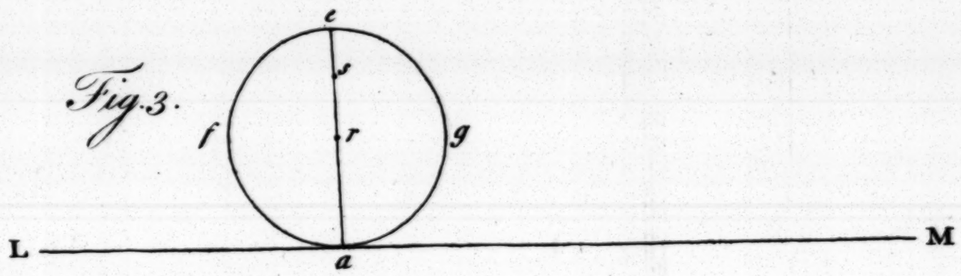


Fig. 4.

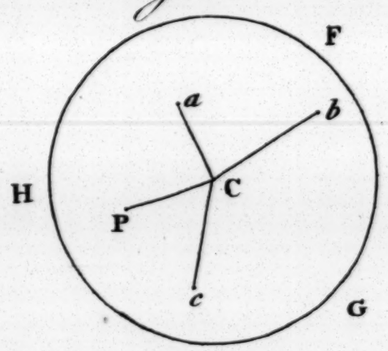
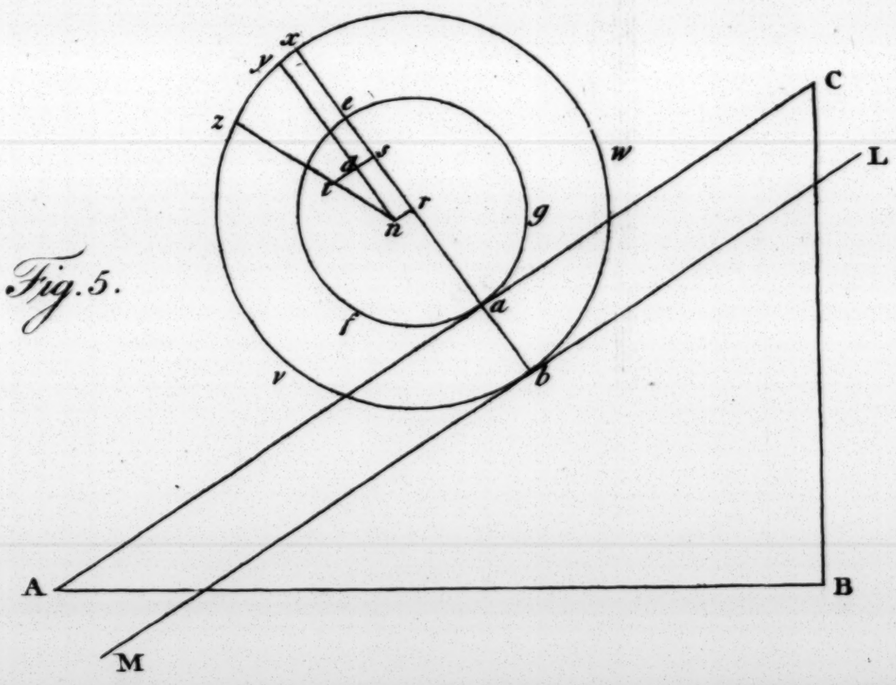


Fig. 5.



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$\frac{rs}{rx} \times nr + nr) = \frac{rs+rx}{rx} \times nr$, conſequently $\frac{rs+rx}{rx} : 1 :: ts : nr ::$ (from what is proved above) $m \times \mathrm{CB} : a \times \mathrm{CA}$; therefore $a \times \mathrm{CA} \times rs + a \times \mathrm{CA} \times rx = m \times \mathrm{CB} \times rx$, hence $rx = \frac{a \times \mathrm{CA} \times rs}{m \times \mathrm{CB} - a \times \mathrm{CA}}$ the radius of the circle which rolling down the inclined plane LM, and carrying the body with it, will give the true ratio of its progreſſive to its rotatory motion, and conſequently that point of the circle which coincides with any given point of the body will, as the circle revolves upon the line LM, deſcribe the ſame curve as the correſponding point of the body; but as the nature of the curve deſcribed by any point of a circle revolving upon a ſtraight line is already very well known, it ſeems unneceſſary to give the inveſtigation.

By a method of reaſoning, not very different, may the nature of the curve, which is deſcribed by any point of a body moving upon an horizontal plane, and affected by friction, be determined.

XI. *Obſervations and Experiments on the Light of Bodies in a State of Combuſtion. By the Rev. Mr.* Morgan; *communicated by the Rev.* Richard Price, *LL.D. F.R.S.*

Read January 27, 1785.

THE diſcuſſion which I now wiſh to lay before the Royal Society is nothing more than a ſeries of facts, and of concluſions which ſeem to flow from thoſe facts, and from an attention to the following data.

I. That light is a body, and like all other bodies ſubject to the laws of attraction.

II. That light is an heterogeneous body, and that the ſame attractive power operates with different degrees of force on its different parts.

III. That the light which eſcapes from combuſtibles when decompoſed by heat, or by any other means, was, previous to its eſcape, a component part of thoſe ſubſtances.

It is an obvious concluſion from theſe data, that when the attractive force, by which the ſeveral rays of light are attached to a body, is weakened, ſome of thoſe rays will eſcape

eſcape ſooner than others. Thoſe which are united with the leaſt degree of power will eſcape firſt, and thoſe which adhere to it moſt ſtrongly will (if I may be allowed the expreſſion) be the laſt to quit their baſis. We may here have recourſe to a familiar fact, which is analogous to this, and will illuſtrate it. If a mixture, conſiſting of equal parts of water, of ſpirits of wine, and of other more fixed bodies, be placed over a fire; the firſt influence of that heat, to which all the ingredients are alike expoſed, will carry off the ſpirits of wine only. The next will carry off the ſpirits of wine blended with particles of water. A ſtill greater degree of heat will blend with the vapour which eſcapes a part of the more fixed bodies, till at length what evaporates will be a mixture of all the ingredients which were at firſt expoſed to the fire. In like manner, when the ſurface of a combuſtible is in a ſtate of decompoſition, thoſe parts which are the leaſt fixed, or which are united to it with the leaſt force, will be ſeparated firſt. Amongſt theſe the indigo rays of light will make the earlieſt appearance. By increaſing the heat we ſhall mix the violet with the indigo. By increaſing it ſtill more we ſhall add the blue and the green to the mixture, till at length we reach that intenſity of heat which will cauſe all the rays to eſcape at the ſame inſtant, and make the flame of a combuſtible perfectly white. It is not my preſent deſign to ſhew why the moſt refrangible rays are the firſt which eſcape from a burning body, but to enumerate the ſeveral facts which ſeem to ſhew, that ſuch a general law takes place in combuſtion; and that the various colours of bodies in this ſtate are uniformly regulated by that decreaſe of attractive force now deſcribed.

By

By examining the flame of a common candle we may obſerve, that its loweſt extremities, or the part in which the black colour of the wick terminates, diſcharges the leaſt heat; and that, as the vertex of the flame is approached, a ſucceſſive order of parts is paſſed through, in which the loweſt is continually adding to the heat of what is juſt above it, till we come to the top of the flame, near which all the heat is collected into a focus. At the loweſt extremity, however, where the heat is inconſiderable, a blue colour may be always obſerved; and from this appearance, amongſt others, it may, I think, be ſafely concluded, that the blue rays are ſome of thoſe which eſcape from combuſtibles in an early period of their decompoſition; and that if the decompoſition could be examined in a period ſtill more early, the colour of their flame would be violet. By an *a priori* deduction of this kind, I was led to watch the appearances of a candle more attentively; whence I found that to the external boundary of a common candle is annexed a filament of light, which, if proper care be taken to prevent the eſcape of too much ſmoke, will appear moſt beautifully coloured with the violet and indigo rays. To the preceding inſtance of a common candle many facts may be added, which ſpeak a ſimilar language. If ſulphur or æther is burned, or any of thoſe combuſtibles whoſe vapour is kindled in a ſmall degree of heat, a blue flame will appear, which, if examined by the priſm, will be found to conſiſt of the violet, the indigo, the blue, and ſometimes a ſmall quantity of the green rays. The beſt mode, however, of ſhewing the eſcape of ſome rays by that degree of heat which will not ſeparate others till increaſed, is the following. Give a piece of brown paper a ſpherical form, by preſſing it upon

upon any hard globular ſubſtance. Gradually bring the paper, thus formed, to that diſtance from the candle at which it will begin to take fire. In this caſe a beautiful blue flame may be ſeen, hanging as it were by the paper till a hole is made in it, when the flame, owing to the increaſed action of the air upon all parts of it, becomes white, though the edges ſtill continue of a blue or violet colour. As a confirmation of what I have concluded from the preceding facts, it may be obſerved, that the very flame which, when expoſed to a certain degree of heat, emitted the moſt refrangible rays only, will, if expoſed to a greater degree of heat, emit ſuch as are leſs refrangible. The flames of ſulphur, ſpirits of wine, &c. when ſuddenly expoſed to the heat of a reverberatory, change their blue appearance for that which is perfectly white. But to gain a more ſtriking diverſity of this fact, I adopted Mr. MELVILL's mode of examining bodies whilſt on fire. I darkened my room, and placed between my eye and the combuſtible a ſheet of paſteboard, in the center of which I made a ſmall perforation. As the light of the burning body eſcaped through this perforation, I examined it with a priſm, and obſerved the following appearances. When the ſpirits of wine were ſet on fire, all the rays appeared in the perforation; but the violet, the blue, and the green, in the greateſt abundance. When the combuſtion of the ſpirits was checked by throwing ſome ſal ammoniac into the mixture, the red rays diſappeared; but when, by the long continuance of the flame, the ſal ammoniac was rendered ſo hot as to increaſe, rather than diminiſh the combuſtion, the red rays again appeared at the perforation. If the ſcreen was managed ſo that the different parts of the flame might be examined ſeparately, I always obſerved that

the colours varied according to the degree of heat. At the baſe of the flame, or where the heat was leaſt, the indigo, the violet, and a very ſmall tinge of the blue and green appeared. As I approached the vertex of the flame, the rays which eſcaped became more and more numerous till I reached the top, when all the rays appeared in the priſm. It ſhould be attended to, that when the red rays firſt made their appearance, their quantity was ſmall, and gradually increaſed as the eye in its examination approached that part where the heat was greateſt. Mr. MELVILL, when he made ſome of the preceding experiments, obſerved, that the yellow rays frequently eſcaped in the greateſt abundance; but this ſingularity proceeded from ſome circumſtances which eſcaped his attention. In conſequence of mixing acids or ſalts with the burning ſpirits, a very denſe fume of unignited particles ariſes, and before the rays of the burning body arrive at the perforation where the priſm catches them, they muſt paſs through a medium which will abſorb a great part of the indigo and the violet. On the other hand, owing to the imperfection of the decompoſition, very few of the red rays are ſeparated from their baſis, and conſequently the yellow and the orange rays are thoſe alone which paſs through the unburnt ſmoke of the flame.

I would now proceed with obſerving, that, beſides the increaſe or decreaſe of heat, there are other modes of retarding or accelerating the combuſtion of bodies, by which alſo may be examined ſome of the preceding illuſtrations.

1. A candle burns moſt rapidly and brilliantly in dephlogiſticated air.

2. The

2. The blue colour of a ſulphureous flame in pure air is changed into a dazzling white.

3. The flame of inflammable air, when mixed with nitrous air, is green. It is white ſtrongly tinged with the indigo and violet when mixed with common air; but when mixed with dephlogiſticated air, or ſurrounded by it, the brilliancy of its flame is moſt ſingularly beautiful.

If the preceding facts prove that light, as an heterogeneous body, is gradually decompoſed during combuſtion; if they prove, likewiſe, that the indigo rays eſcape with the leaſt heat, and the red with the greateſt; I think we may rationally account for ſeveral ſingularities in the colours of different flames. If a piece of paper, impregnated with a ſolution of copper in the nitrous acid, be ſet on fire, the bottom and ſides of the flame are always tinged with green. Now this flame is evidently in that weak ſtate of decompoſition, in which the moſt refrangible rays eſcape in the greateſt abundance; but of theſe rays the green eſcape moſt plentifully through the unignited vapour and that portion of the atmoſphere which ſeparates the eye from the flame. The peculiarity which I have now endeavoured to account for may be obſerved in the greateſt perfection in braſs founderies. The heat in this inſtance, though very ſtrong, is ſcarcely adequate to the decompoſition of the metallic vapour which eſcapes from the melted braſs. A very ſingular flame therefore appears to the eye; for while its edges are green, its body is ſuch as to give the objects around a very pallid or ghaſtly appearance, which is the conſequence of its wanting that portion of red rays which is neceſſary to make a perfect white.

The moſt ſingular phænomenon attending a burning body is, perhaps, the red appearance it aſſumes in its laſt ſtage of combuſtion. The preceding facts and obſervations may, I think, help us to explain it.

1. After a body has continued to burn for ſome time, its external ſurface is to be regarded as having loſt a great portion if not the whole of thoſe rays which the firſt application of heat was able to ſeparate. But theſe rays were the indigo, the violet, the blue, and perhaps the green. Nothing, therefore, will remain to be ſeparated, but the yellow, the orange, and the red. Conſequently, the combuſtion of the body, in its laſt ſtate of decompoſition, can aſſume no other than a reddiſh appearance. But

2. Let us conſider the external ſurface of the combuſtible as annexed to an inner ſurface, which may be partly, but not ſo perfectly decompoſed as itſelf: for the violence of the heat will be found to leſſen in its effects the nearer it approaches to the center of the ſubſtance which is expoſed to it. Hence we are to conſider the parts which are juſt covered by the external ſurface as having loſt leſs of their component light than the external ſurface itſelf. Or the former may retain the green rays when the latter has loſt both indigo, violet, blue, and green.

3. Thoſe parts which are nearer the center of the body than either of the preceding muſt, as they are further from the greateſt violence of the heat, have loſt proportionably fewer of their rays. Or while the more external parts may have loſt all but the red, theſe may have loſt only the indigo and violet.

4. The moſt central parts may be unaffected by the heat; and whenever the fire does reach theſe parts, they will immediately diſcharge their indigo rays, and be decompoſed in the gradual

gradual manner which I have already deſcribed. A piece of rotten wood, whilſt burning, will exemplify and confirm the preceding illuſtration. When influenced by the external air only, if examined through a priſm, no rays will be found to eſcape but the orange and the red. By blowing upon the burning wood with a pair of bellows, the combuſtion, being increaſed, will affect thoſe internal parts of the body which were not acted upon before. Theſe parts, therefore, will begin to loſe their light, and a priſm will ſhew the green, the blue, the violet, and indigo, all appearing in ſucceſſion. Appearances ſimilar to the preceding may be obſerved in a common kitchen fire. When it is fainteſt, its colour is moſt red, the other rays having been emitted, and the combuſtion at a ſtand; but by blowing upon it in this ſtate, its brightneſs will be increaſed, and more and more of the rays which are yielded by the internal parts of the body will come to the eye, till at length, by continuing to blow, the combuſtion will be made ſo complete as to yield all the rays, or to make it appear perfectly white.

Many are the varieties diſcoverable in the flames and in the appearances of fixed burning bodies to which the preceding obſervations may be applied; but, to avoid unneceſſary amplification I will take notice only of what appears to me an imperfection in Sir ISAAC NEWTON's definition of flame. He conjectures, that it may be a vapour heated red-hot. I think I ſhould rather ſay, that flame is an inſtance of combuſtion whoſe colour will be determined by the degree of decompoſition which takes place. If it be very imperfect, the moſt refrangible rays only will appear. If it be very perfect, all the rays will appear, and its flame will be brilliant in-

proportion

proportion to this perfection. There are flames, however, which conſiſt of burning particles, whoſe rays have partly eſcaped before they aſcended in the form of vapour. Such would be the flame of a red-hot coal, if expoſed to ſuch a heat as would gradually diſperſe it into vapour. When the fire is very low under the furnace of an iron foundery, at the upper orifice of the chimney a red flame of this kind may be ſeen, which is different from the flame that appears immediately after freſh coals have been thrown upon the fire; for, in conſequence of adding ſuch a ſupply to the burning fuel, a vaſt column of ſmoke aſcends, and forms a medium ſo thick as to abſorb moſt of the rays excepting the *red*.

Experiments on electric light.

If we would wiſh to procure any degree of certainty in any hypotheſis which we may form concerning electrical light, perhaps the following general deductions may be of ſome ſervice to us.

1. There is no fluid or ſolid body in its paſſage through which the electric fluid may not be made luminous. In water, ſpirits, oil, animal fluids of all kinds, the diſcharge of a Leyden phial of almoſt any ſize will appear very ſplendid, provided we take care to place them in the circuit, ſo that the fluid may not paſs through too great a quantity of them. My general method is to place the fluid, on which I mean to make the experiment, in a tube three-quarters of an inch in diameter, and four inches long. I ſtop up the orifices of the tube with two corks, through which I puſh two pointed wires, ſo that the points may approach within one-eighth of an inch to each

other. The fluid in paſſing through the interval which ſeparates the wires is always luminous, if a force be uſed ſufficiently ſtrong. I ſhould obſerve, that the glaſs tube, if not very thick, always breaks when this experiment ſucceeds. To make the paſſage of the fluid luminous in the acids, they muſt be placed in capillary tubes, and two wires introduced, as in the preceding experiment, whoſe points ſhall be very near each other. It is a well known fact, that the diſcharge of a ſmall Leyden phial in paſſing over a ſtrip of gold, ſilver, or Dutch metal leaf, will appear very luminous. By conveying the contents of a jar, meaſuring two gallons, over a ſtrip of gold leaf one-eighth of an inch in diameter, and a yard long, I have frequently given the whole a dazzling brightneſs. I cannot ſay, that a much greater length might not have been made very ſplendid, nor can I determine to what length the force of a battery might be made luminous in this manner. We may give this experiment a curious diverſity, by laying the gold or ſilver leaf on a piece of glaſs, and then placing the glaſs in water; for the whole gold leaf will appear moſt brilliantly luminous in the water by expoſing it, thus circumſtanced, to the exploſion of a battery.

2. The difficulty of making any quantity of the electrical fluid luminous in any body increaſes as the conducting power of that body increaſes.

EXP. I. In order to make the contents of a jar luminous in boiling water, a much higher charge is neceſſary than would be ſufficient to make it luminous in cold water, which is univerſally allowed to be the worſt conductor.

EXP. II. I have various reaſons for believing the acids to be very good conductors. If therefore into a tube, filled with water, and circumſtanced as I have already deſcribed, a few

drops

drops of either of the mineral acids are poured, it will be almoſt impoſſible to make the fluid luminous in its paſſage through the tube.

EXP. III. If a ſtring *, whoſe diameter is one-eighth of an inch and whoſe length is ſix or eight inches, is moiſtened with water, the contents of a jar will paſs through it luminouſly, but no ſuch appearance can be produced by any charge of the ſame jar, provided the ſame ſtring be moiſtened with one of the mineral acids. To the preceding inſtance we may add the various inſtances of metals which will conduct the electrical fluid without any appearance of light, in circumſtances the ſame with thoſe in which the ſame force would have appeared luminous in paſſing through other bodies whoſe conducting power is leſs. But I proceed to obſerve,

III. That the eaſe with which the electrical fluid is rendered luminous in any particular body is increaſed by increaſing the rarity of the body. The appearance of a ſpark, or of the diſcharge of a Leyden phial, in rarefied air is well known. But we need not reſt the truth of the preceding obſervation on the ſeveral varieties of this fact; ſimilar phænomena attend the rarefaction of æther, of ſpirits of wine, and of water.

EXP. IV. Into the orifice of a tube, 48 inches long, and two-thirds of an inch in diameter, I cemented an iron ball, ſo as to bear the weight which preſſed upon it when I filled the tube with quickſilver, leaving only an interval at the open end, which contained a few drops of water. Having inverted the tube, and plunged the open end of it into a baſon of mercury, the mercury in the tube ſtood nearly half an inch lower than it

* The thickneſs and diameter of the ſtring ſhould be regulated by the force we employ.

did

did in a barometer at the ſame inſtant, owing to the vapour which was formed by the water. But through this rarefied water the electrical ſpark paſſed as luminouſly as it does through air equally rarefied.

EXP. V. If, inſtead of water, a few drops of ſpirits of wine are placed on the ſurface of the mercury, phænomena ſimilar to thoſe of the preceding experiment will be diſcovered, with this difference only, that as the vapour in this caſe is more denſe, the electrical ſpark in its paſſage through it is not quite ſo luminous as it is in the vapour of water.

EXP. VI. Good æther ſubſtituted in the room of the ſpirits of wine will preſs the mercury down ſo low as the height of 16 or 17 inches. The electrical fluid in paſſing through this vapour (unleſs the force be very great indeed) is ſcarcely luminous. But if the preſſure on the ſurface of the mercury in the baſon be gradually leſſened by the aid of an air-pump, the vapour will become more and more rare, and the electric ſpark in paſſing through it more and more luminous.

EXP. VII. I could not diſcover that any vapour eſcaped from the mineral acids when expoſed *in vacuo*. To give them, therefore, greater rarity or tenuity, I found different methods neceſſary. With a fine camel-hair pencil, dipped in the vitriolic, the nitrous, or the marine acid, I drew upon a piece of glaſs a line about one-eighth of an inch broad. In ſome inſtances I extended this line to the length of 27 inches, and found that the contents of an electric battery, conſiſting of 10 pint phials coated, would paſs over the whole length of this line with the greateſt brilliancy. If by widening the line, or by laying on a drop of the acid, its quantity was increaſed in any particular part, the charge, in paſſing through that part, never appeared luminous. Water, ſpirits of wine, circum-

ſtanced ſimilarly to the acids in the preceding experiment, were attended with ſimilar, but not equal effects, becauſe, in conſequence of the inferiority of their conducting power, it was neceſſary to make the line through which the charge paſſed conſiderably ſhorter.

4. The brilliancy or ſplendor of the electric fluid in its paſſage through any body is always increaſed by leſſening the dimenſions of that body. I would explain my meaning by ſaying, that a ſpark, or the diſcharge of a battery which we might ſuppoſe equal to a ſphere one quarter of an inch in diameter, would appear much more brilliant if the ſame quantity of fluid is compreſſed into a ſphere one-eighth of an inch in diameter. This obſervation is the obvious conſequence of many known facts. If the machine be large enough to afford a ſpark whoſe length is nine or ten inches, this ſpark may be ſeen ſometimes forming itſelf into a bruſh, in which ſtate it occupies more room, but appears very faintly luminous. At other times the ſame ſpark may be ſeen dividing itſelf into a variety of ramifications which ſhoot into the ſurrounding air. In this caſe, likewiſe, the fluid is diffuſed over a large ſurface, and in proportion to the extent of that ſurface, ſo is the faintneſs of the appearance. A ſpark, which in the open air cannot exceed one quarter of an inch in diameter, will appear to fill the whole of an exhauſted receiver four inches wide and eight inches long. But in the former caſe it is brilliant, and in the latter it grows fainter and fainter as the ſize of the receiver increaſes. To prove the obſervation, which I think may be juſtified by the preceding facts, I made the following experiments.

EXP. VIII. To an inſulated ball, four inches in diameter, I fixed a ſilver thread, about four yards long. This thread, at the end which was remoteſt from the ball, was fixed to another

inſulated ſubſtance. I brought the ball within the ſtriking diſtance of my conductor, and the ſpark in paſſing from the conductor to the ball appeared very brilliant; but the whole length of the ſilver thread appeared faintly luminous at the ſame inſtant. In other words, when the ſpark was confined within the dimenſions of a ſphere one-eighth of an inch in diameter, it was bright, but, when diffuſed over the ſurface of air which received it from the thread, its light became ſo faint as to be ſeen only in a dark room. If I leſſened the ſurface of air which received the ſpark by ſhortening the thread, I never failed to increaſe the brightneſs of the appearance.

EXP. IX. To prove that the faintneſs of the electric light *in vacuo* depends on the enlarged dimenſions of the ſpace through which it is diffuſed, we have nothing more to do than to introduce two pointed wires into the vacuum, ſo that the fluid may paſs from the point of the one to the point of the other, when the diſtance between them is not more than the one-tenth of an inch. In this caſe we ſhall find a brilliancy as great as in the open air.

EXP. X. Into a Torricellian vacuum, 36 inches in length, I conveyed as much air as would have filled two inches only of the exhauſted tube, if it were inverted in water. This quantity of air afforded reſiſtance enough to condenſe the fluid as it paſſed through the tube into a ſpark 38 inches in length. The brilliancy of the ſpark in condenſed air, in water, and in all ſubſtances through which it paſſes with difficulty, depends on principles ſimilar to thoſe which account for the preceding facts. I would now proceed to ſhew,

5. That in the appearances of electricity, as well as in thoſe of burning bodies, there are caſes in which all the rays of light do not eſcape; and that the moſt refrangible rays are thoſe

which eſcape firſt or moſt eaſily. The electrical bruſh is always of a purple or bluiſh hue. If you convey a ſpark through a Torricellian vacuum, made * *without* boiling the mercury in the tube, the bruſh will diſplay the indigo rays. The ſpark, however, may be divided and weakened even in the open air, ſo as to yield the moſt refrangible rays only.

EXP. XI. To an inſulated metallic ball, four inches in diameter, I fixed a wire a foot and a half long. This wire terminated in four ramifications, each of which was fixed to a metallic ball half an inch in diameter, and placed at an equal diſtance from a metallic plate, which communicated by metallic conductors with the ground. A powerful ſpark, after falling on the large ball at one extremity of the wire, was divided in its paſſage from the four ſmall balls to the metallic plate. When I examined this diviſion of the fluid in a dark room, I diſcovered ſome little ramifications which yielded the indigo rays only: indeed, at the edges of all weak ſparks the ſame purple appearance may be diſcovered. We may likewiſe obſerve, that the nearer we approach the center of the ſpark, the greater is the brilliancy of its colour. But I would now wiſh to ſhew

6. That the influence of different media on electrical light is analogous to their influence on ſolar light, and will help us to account for ſome very ſingular appearances.

EXP. XII. Let a pointed wire, having a metallic ball fixed to one of its extremities, be forced obliquely into a piece of wood, ſo as to make a ſmall angle with the ſurface of the wood, and to make

* If the Torricellian vacuum is made with mercury perfectly purged of air, it becomes a perfect non-conductor. This, I believe, will be proved decifively by ſome experiments which I hope will be ſoon communicated to the Royal Society.

Dr. PRICE.

the point lie about one-eighth of an inch below the ſurface. Let another pointed wire, which communicates with the ground, be forced in the ſame manner into the ſame wood, ſo that its point likewiſe may lie about one-eighth of an inch below the ſurface, and about two inches diſtant from the point of the firſt wire. Let the wood be inſulated, and a ſtrong ſpark which ſtrikes on the metallic ball will force its paſſage through the interval of wood which lies between the points, and appear as red as blood. To prove that this appearance depends on the wood's abſorption of all the rays but the red, I would obſerve, that the greater the depth of the points is below the ſurface, the leſs mixed are the red rays. I have been able ſometimes, by increaſing or diminiſhing the depth of the points, to give the ſpark the following ſucceſſion of colours. When they were deepeſt below the ſurface, the red only came to the eye through a priſm. When they were raiſed a little nearer the ſurface, the red and orange appeared. When nearer ſtill, the yellow; and ſo on till, by making the ſpark paſs through the wood very near its ſurface, all the rays were at length able to reach the eye. If the points be only one-eighth of an inch below the ſurface of ſoft deal wood, the red, the orange, and the yellow rays will appear as the ſpark paſſes through it. But when the points are at an equal depth in a harder piece of wood (ſuch as box) the yellow, and perhaps the orange, will diſappear. As a farther proof that the phænomena I am deſcribing are owing to the interpoſition of the wood, as a medium which abſorbs ſome of the rays and ſuffers others to eſcape, it may be obſerved, that when the ſpark ſtrikes very brilliantly on one ſide of the piece of deal, on the other ſide it will appear very red. In like manner a red appearance may be given to a ſpark which ſtrikes

brilliantly

brilliantly over the inſide of a tube, merely by ſpreading ſome pitch very thinly over the outſide of the ſame tube.

EXP. XIII. I would now give another fact, whoſe ſingularities depend very much on the influence of the medium through which the electrical light is made to paſs. If into a Torricellian vacuum, of any length, a few drops of æther are conveyed, and both ends of the vacuum are ſtopped up with metallic conductors, ſo that a ſpark may paſs through it, the ſpark in its paſſage will aſſume the following appearances. When the eye is placed cloſe to the tube, the ſpark will appear perfectly white. If the eye is removed to the diſtance of two yards, it will appear green; but at the diſtance of ſix or ſeven yards, the colour of the ſpark will be reddiſh. Theſe changes evidently depend on the quantity of medium through which the the light paſſes; and the red light more particularly, which we ſee at the greateſt diſtance from the tube, is accounted for on the ſame principle as the red light of a diſtant candle or a beclouded ſun.

EXP. XIV. Dr. PRIESTLEY long ago obſerved the red appearance of the ſpark when paſſing through inflammable air. But this appearance is very much diverſified by the quantity of medium, through which you look at the ſpark. When at a very conſiderable diſtance, the red comes to the eye unmixed; but, if the eye is placed cloſe to the tube, the ſpark appears white and brilliant. In confirmation, however, of ſome of my concluſions, I would obſerve, that by increaſing the quantity of fluid which is conveyed through any portion of inflammable air, or by condenſing that air, the ſpark may be entirely deprived of its red appearance, and made perfectly brilliant. I have only to add, that all weak exploſions and

ſparks,

ſparks, when viewed at a diſtance, bear a reddiſh hue. Such are the exploſions which have paſſed through water, ſpirits of wine, or any bad conductor, when confined in a tube whoſe diameter is not more than an inch. The reaſon of theſe appearances ſeems to be, that the weaker the ſpark or exploſion is, the leſs is the light which eſcapes; and the more viſible the effect of any medium which has a power to abſorb ſome of that light.

The preceding obſervations concerning electrical light were the reſult of my attempts to arrange, under general heads, the principal ſingularities attending it. They may, perhaps, aſſiſt others in determining how far they may have led my mind aſtray in giving birth to a theory which I would now briefly deſcribe in a few queries.

I. If we conſider all bodies as compounds, whoſe conſtituent parts are kept together by attracting one another with different forces, can we avoid concluding, that the operations of that attractive force are regulated, not only by the quality, but the quantity likewiſe of thoſe component parts? If an union of a certain number of one kind of particles, with a certain number of a ſecond and third kind of particles, forms a particular body, muſt not the bond which keeps that body together be weakened or ſtrengthened by increaſing or diminiſhing any one of the different kinds of particles which enter into its conſtitution?

II. When, to the natural ſhare of the electric fluid already exiſting in the body, a freſh quantity of the ſame fluid is added, muſt not ſome of the component parts of that body eſcape; or muſt not that attractive force which kept all together be ſo far weakened as to let looſe ſome conſtituent

parts,

parts, and amongſt theſe the particles of light in particular?

III. Muſt not this ſeparation of parts be great in proportion to the quantity of extraneous particles which are added to the body? Or (agreeable to the 4th obſervation) muſt not the ſpark be more ſplendid and brilliant, the more the electrical fluid is concentered in any given ſpace?

IV. In the diminution or alteration of that attractive force on which depends the conſtitution of bodies, may there not be a gradation which, in the preſent caſe, as well as in that of burning bodies, will cauſe the eſcape of ſome rays ſooner than others?

Obſervations on phoſphoric light.

It is obvious, from Mr. B. WILSON's experiments, that there are many curious diverſities in the appearances of phoſphori. Some ſhells, prepared agreeably to his directions, after expoſure to the ſun or to the flaſh of a battery, emit a purple, others a green, and others a reddiſh light. If with Mr. WILSON we ſuppoſe, that theſe ſhells are in a ſtate of ſlow combuſtion, may we not conclude, that ſome are juſt beginning to burn, and therefore, agreeably to what I have obſerved on combuſtible bodies, emitting the moſt refrangible rays; whilſt others are in a more advanced ſtate of combuſtion, and therefore emitting the leaſt refrangible. If this concluſion be right, the ſhells which are emitting the purple, or the green, muſt ſtill retain the yellow, the orange, and the red, which will alſo make their appearance as ſoon as the combuſtion is ſufficiently increaſed.

EXP.

EXP. XV. Place a ſhell whilſt emitting its green rays on a warm ſhovel, and the appearance of the ſhell will be ſoon changed into that of a yellow mixed with red. To Mr. WILSON's theory, however, of ſlow combuſtion the following objections may be oppoſed.

1°. If phoſphoric ſhells owe their light to this cauſe, we muſt conſider the word combuſtion when applied to them as implying in its ſignification all thoſe circumſtances which are the uſual attendants of a body whilſt on fire. Amongſt other neceſſary conſequences in ſuch a caſe, the increaſe of heat muſt increaſe the decompoſition of the combuſtible; whereas we diſcover an effect the very oppoſite to this in the appearance of a phoſphoric body, which never fails to loſe its light entirely in a certain degree of heat, without loſing the power of becoming phoſphoric again when it has been ſufficiently cooled. Beſides, when a phoſphoric ſhell has been made very hot, and while it has continued ſo, I have conveyed the moſt brilliant diſcharge of a battery over it without effect. In other words, heat, or the very cauſe which promotes combuſtion in all other inſtances, in this particular caſe puts an end to it. Mr. WILSON, in his Treatiſe on Phoſphori, has deſcribed an experiment ſimilar to the preceding. But the reſult he mentions is different from that here mentioned. However, from a regard to his authority, I have ſo frequently repeated my trials that I cannot juſtly ſuſpect myſelf of any inaccuracy. 2°. When bodies are waſted by combuſtion, they can never be made to re-aſſume the appearances which they previouſly diſplayed. No power can give to aſhes the phænomena of a burning coal. But phoſphoric bodies are very different in this reſpect; for a ſhell may be made to loſe all its light by expoſure to heat, and again

may be made as luminous as ever by expoſure to the ſun. But 3°. It is obſervable, that ſome bodies, which are moſt beautifully phoſphoric, or which, according to Mr. WILSON's theory, are in the beſt ſtate of ſlow combuſtion; it is obſervable, I ſay, that the ſame bodies are the moſt obſtinate in reſiſting the fire. The diamond, which to be decompoſed requires the force of a moſt powerful furnace, is, according to this theory, waſting away, owing to a ſeparation of parts which is promoted by the weakeſt influence of the ſun's rays.—Without determining whether the preceding objections be valid, let us now ſee the conſequence of admitting the common hypotheſis, that the detention of thoſe rays which fall upon phoſphori is owing to ſome force which prevents their immediate reflection, but is not adequate to their entire abſorption. This force, whatever it be, cannot well be ſuppoſed to operate with equal power on all the rays. And if this be not the caſe, I think we cannot avoid concluding, that phoſphoric ſhells will aſſume different colours, owing to the earlier and later eſcape of the different rays of light. This concluſion is juſtified by an experiment which I have already appealed to. When the force is ſuch as to admit of the eſcape of the purple, the blue, and the green, we have only to leſſen that force by warming the body, and the yellow, the orange, and red eſcape. It is proved by BECCARIA's extenſive experience on this ſubject, that there is ſcarcely any body which is not phoſphoric, or which may not be made ſo by heat. But as the phoſphoric force is moſt powerful when the purple rays only eſcape, ſo we are to conclude, that it is weakeſt when it is able to retain the red rays only. This concluſion is agreeable to ſeveral facts. Chalk, oyſter-ſhells, together with thoſe phoſphoric bodies whoſe goodneſs

has been very much impaired by long keeping; when finely powdered and placed within the circuit of an electrical battery, will exhibit by their ſcattered particles a ſhower of light; but theſe particles will appear reddiſh, or their phoſphoric power will be ſufficient only to detain the yellow, orange, and red rays. When ſpirits of wine are in a ſimilar manner brought within the circuit of a battery, a ſimilar effect may be diſcovered; its particles diverge in ſeveral directions, diſplaying a moſt beautiful golden appearance. The metallic calces are, of all bodies, thoſe which are rendered phoſphoric with the greateſt difficulty. But even theſe may be ſcattered into a ſhower of red luminous particles by the electric ſtroke.

Norwich, Oct. 7, 1784.

POSTSCRIPT by the Rev. Dr. PRICE.

BY the *phoſphoric* force mentioned in the laſt paragraph of this paper, Mr. MORGAN appears to mean, not the force with which a phoſphoric body *emits*, but the force with which it *abſorbs* and *retains* light. This laſt force is proportioned to the degree of attraction between the phoſphoric body and light; and therefore muſt (as Mr. MORGAN obſerves) be *weakeſt* when it emits ſo freely the light it has imbibed as not to retain thoſe rays which adhere to it moſt ſtrongly. According to Mr. MORGAN's theory, theſe rays are thoſe which

are leaſt refrangible. The obſervations and experiments in this paper ſeem to render this theory probable. It is, however, an objection to it, that the leſs refrangibility of rays ſeems to imply a leſs force of attraction between them and the ſubſtances which refract them; but it ſhould be conſidered, that, poſſibly, the force of coheſion, which unites the rays of light to bodies, may be a different power from that which refracts them.

XII. *On the Conſtruction of the Heavens.*

By William Herſchel, *Eſq. F. R. S.*

Read February 3, 1785.

THE ſubject of the Conſtruction of the Heavens, on which I have ſo lately ventured to deliver my thoughts to this Society, is of ſo extenſive and important a nature, that we cannot exert too much attention in our endeavours to throw all poſſible light upon it; I ſhall, therefore, now attempt to purſue the delineations of which a faint outline was begun in my former paper.

By continuing to obſerve the heavens with my laſt conſtructed, and ſince that time much improved inſtrument, I am now enabled to bring more confirmation to ſeveral parts that were before but weakly ſupported, and alſo to offer a few ſtill further extended hints, ſuch as they preſent themſelves to my preſent view. But firſt let me mention that, if we would hope to make any progreſs in an inveſtigation of this delicate nature, we ought to avoid two oppoſite extremes, of which I can hardly ſay which is the moſt dangerous. If we indulge a fanciful imagination and build worlds of our own, we muſt not wonder at our going wide from the path of truth and nature; but theſe will vaniſh like the Carteſian vortices, that ſoon gave way when better theories were offered. On the other hand, if we add obſervation to obſervation, without attempting to draw not only certain concluſions, but alſo conjectural

views

views from them, we offend againſt the very end for which only obſervations ought to be made. I will endeavour to keep a proper medium; but if I ſhould deviate from that, I could wiſh not to fall into the latter error.

That the milky way is a moſt extenſive ſtratum of ſtars of various ſizes admits no longer of the leaſt doubt; and that our ſun is actually one of the heavenly bodies belonging to it is as evident. I have now viewed and gaged this ſhining zone in almoſt every direction, and find it compoſed of ſtars whoſe number, by the account of theſe gages, conſtantly increaſes and decreaſes in proportion to its apparent brightneſs to the naked eye. But in order to develop the ideas of the univerſe, that have been ſuggeſted by my late obſervations, it will be beſt to take the ſubject from a point of view at a conſiderable diſtance both of ſpace and of time.

Theoretical view.

Let us then ſuppoſe numberleſs ſtars of various ſizes, ſcattered over an indefinite portion of ſpace in ſuch a manner as to be almoſt equally diſtributed throughout the whole. The laws of attraction, which no doubt extend to the remoteſt regions of the fixed ſtars, will operate in ſuch a manner as moſt probably to produce the following remarkable effects.

Formation of nebulæ.

Form I. In the firſt place, ſince we have ſuppoſed the ſtars to be of various ſizes, it will frequently happen that a ſtar, being conſiderably larger than its neighbouring ones, will attract them more than they will be attracted by others that are immediately

immediately around them; by which means they will be, in time, as it were, condenſed about a center; or, in other words, form themſelves into a cluſter of ſtars of almoſt a globular figure, more or leſs regularly ſo, according to the ſize and original diſtance of the ſurrounding ſtars. The perturbations of theſe mutual attractions muſt undoubtedly be very intricate, as we may eaſily comprehend by conſidering what Sir ISAAC NEWTON ſays in the firſt book of his Principia, in the 38th and following problems; but in order to apply this great author's reaſoning of bodies moving in ellipſes to ſuch as are here, for a while, ſuppoſed to have no other motion than what their mutual gravity has imparted to them, we muſt ſuppoſe the conjugate axes of theſe ellipſes indefinitely diminiſhed, whereby the ellipſes will become ſtraight lines.

Form II. The next caſe, which will alſo happen almoſt as frequently as the former, is where a few ſtars, though not ſuperior in ſize to the reſt, may chance to be rather nearer each other than the ſurrounding ones; for here alſo will be formed a prevailing attraction in the combined center of gravity of them all, which will occaſion the neighbouring ſtars to draw together; not indeed ſo as to form a regular or globular figure, but however in ſuch a manner as to be condenſed towards the common center of gravity of the whole irregular cluſter. And this conſtruction admits of the utmoſt variety of ſhapes, according to the number and ſituation of the ſtars which firſt gave riſe to the condenſation of the reſt.

Form III. From the compoſition and repeated conjunction of both the foregoing forms, a third may be derived, when many large ſtars, or combined ſmall ones, are ſituated in long extended, regular, or crooked rows, hooks, or branches; for they will alſo draw the ſurrounding ones, ſo as to ptoduce figures

of

of condenſed ſtars coarſely ſimilar to the former which gave riſe to theſe condenſations.

Form IV. We may likewiſe admit of ſtill more extenſive combinations; when, at the ſame time that a cluſter of ſtars is forming in one part of ſpace, there may be another collecting in a different, but perhaps not far diſtant quarter, which may occaſion a mutual approach towards their common center of gravity.

V. In the laſt place, as a natural conſequence of the former caſes, there will be formed great cavities or vacancies by the retreat of the ſtars towards the various centers which attract them; ſo that upon the whole there is evidently a field of the greateſt variety for the mutual and combined attractions of the heavenly bodies to exert themſelves in. I ſhall, therefore, without extending myſelf farther upon this ſubject, proceed to a few conſiderations, that will naturally occur to every one who may view this ſubject in the light I have here done.

Objections conſidered.

At firſt ſight then it will ſeem as if a ſyſtem, ſuch as it has been diſplayed in the foregoing paragraphs, would evidently tend to a general deſtruction, by the ſhock of one ſtar's falling upon another. It would here be a ſufficient anſwer to ſay, that if obſervation ſhould prove this really to be the ſyſtem of the univerſe, there is no doubt but that the great Author of it has amply provided for the preſervation of the whole, though it ſhould not appear to us in what manner this is effected. But I ſhall moreover point out ſeveral circumſtances that do manifeſtly tend to a general preſervation; as, in the firſt place, the indefinite extent of the ſidereal heavens,

which muſt produce a balance that will effectually ſecure all the great parts of the whole from approaching to each other. There remains then only to ſee how the particular ſtars belonging to ſeparate cluſters will be preſerved from ruſhing on to their centers of attraction. And here I muſt obſerve, that though I have before, by way of rendering the caſe more ſimple, conſidered the ſtars as being originally at reſt, I intended not to exclude projectile forces; and the admiſſion of them will prove ſuch a barrier againſt the ſeeming deſtructive power of attraction as to ſecure from it all the ſtars belonging to a cluſter, if not for ever, at leaſt for millions of ages. Beſides, we ought perhaps to look upon ſuch cluſters, and the deſtruction of now and then a ſtar, in ſome thouſands of ages, as perhaps the very means by which the whole is preſerved and renewed. Theſe cluſters may be the *Laboratories* of the univerſe, if I may ſo expreſs myſelf, wherein the moſt ſalutary remedies for the decay of the whole are prepared.

Optical appearances.

From this theoretical view of the heavens, which has been taken, as we obſerved, from a point not leſs diſtant in time than in ſpace, we will now retreat to our own retired ſtation, in one of the planets attending a ſtar in its great combination with numberleſs others; and in order to inveſtigate what will be the appearances from this contracted ſituation, let us begin with the naked eye. The ſtars of the firſt magnitude being in all probability the neareſt, will furniſh us with a ſtep to begin our ſcale; ſetting off, therefore, with the diſtance of Sirius or Arcturus, for inſtance, as unity, we will at preſent ſuppoſe, that thoſe of the ſecond magnitude are at double, and

thoſe of the third at treble the diſtance, and ſo forth. It is not neceſſary critically to examine what quantity of light or magnitude of a ſtar intitles it to be eſtimated of ſuch or ſuch a proportional diſtance, as the common coarſe eſtimation will anſwer our preſent purpoſe as well; taking it then for granted, that a ſtar of the ſeventh magnitude is about ſeven times as far as one of the firſt, it follows, that an obſerver, who is incloſed in a globular cluſter of ſtars, and not far from the center, will never be able, with the naked eye, to ſee to the end of it: for, ſince, according to the above eſtimations, he can only extend his view to about ſeven times the diſtance of Sirius, it cannot be expected that his eyes ſhould reach the borders of a cluſter which has perhaps not leſs than fifty ſtars in depth every where around him. The whole univerſe, therefore, to him will be compriſed in a ſet of conſtellations, richly ornamented with ſcattered ſtars of all ſizes. Or if the united brightneſs of a neighbouring cluſter of ſtars ſhould, in a remarkable clear night, reach his ſight, it will put on the appearance of a ſmall, faint, whitiſh, nebulous cloud, not to be perceived without the greateſt attention. To paſs by other ſituations, let him be placed in a much extended ſtratum, or branching cluſter of millions of ſtars, ſuch as may fall under the III[d] form of nebulæ conſidered in a foregoing paragraph. Here alſo the heavens will not only be richly ſcattered over with brilliant conſtellations, but a ſhining zone or milky way will be perceived to ſurround the whole ſphere of the heavens, owing to the combined light of thoſe ſtars which are too ſmall, that is, too remote to be ſeen. Our obſerver's ſight will be ſo confined, that he will imagine this ſingle collection of ſtars, of which he does not even perceive the thouſandth part, to be the whole contents of the heavens. Allowing him now the uſe of a

common teleſcope, he begins to ſuſpect that all the milkineſs of the bright path which ſurrounds the ſphere may be owing to ſtars. He perceives a few cluſters of them in various parts of the heavens, and finds alſo that there are a kind of nebulous patches; but ſtill his views are not extended ſo far as to reach to the end of the ſtratum in which he is ſituated, ſo that he looks upon theſe patches as belonging to that ſyſtem which to him ſeems to comprehend every celeſtial object. He now increaſes his power of viſion, and, applying himſelf to a cloſe obſervation, finds that the milky way is indeed no other than a collection of very ſmall ſtars. He perceives that thoſe objects which had been called nebulæ are evidently nothing but cluſters of ſtars. He finds their number increaſe upon him, and when he reſolves one nebula into ſtars he diſcovers ten new ones which he cannot reſolve, He then forms the idea of immenſe ſtrata of fixed ſtars, of cluſters of ſtars and of nebulæ (*a*); till, going on with ſuch intereſting obſervations, he now perceives that all theſe appearances muſt naturally ariſe from the confined ſituation in which we are placed. *Confined* it may juſtly be called, though in no leſs a ſpace than what before appeared to be the whole region of the fixed ſtars; but which now has aſſumed the ſhape of a crookedly branching nebula; not, indeed, one of the leaſt, but perhaps very far from being the moſt conſiderable of thoſe numberleſs cluſters that enter into the conſtruction of the heavens.

Reſult of Obſervations.

I ſhall now endeavour to ſhew, that the theoretical view of the ſyſtem of the univerſe, which has been expoſed in the

(*a*) See a former paper on the Conſtruction of the Heavens.

foregoing part of this paper, is perfectly conſiſtent with facts, and ſeems to be confirmed and eſtabliſhed by a ſeries of obſervations. It will appear, that many hundreds of nebulæ of the firſt and ſecond forms are actually to be ſeen in the heavens, and their places will hereafter be pointed out. Many of the third form will be deſcribed, and inſtances of the fourth related. A few of the cavities mentioned in the fifth will be particulariſed, though many more have already been obſerved; ſo that, upon the whole, I believe, it will be found, that the foregoing theoretical view, with all its conſequential appearances, as ſeen by an eye incloſed in one of the nebulæ, is no other than a drawing from nature, wherein the features of the original have been cloſely copied; and I hope the reſemblance will not be called a bad one, when it ſhall be conſidered how very limited muſt be the pencil of an inhabitant of ſo ſmall and retired a portion of an indefinite ſyſtem in attempting the picture of ſo unbounded an extent.

But to proceed to particulars: I ſhall begin by giving the following table of gages that have been taken. In the firſt column is the right aſcenſion, and in the ſecond the north polar diſtance, both reduced to the time of FLAMSTEED's Catalogue. In the third are the contents of the heavens, being the reſult of the gages. The fourth ſhews from how many fields of view the gages were deduced, which have been ten or more where the number of the ſtars was not very conſiderable; but, as it would have taken too much time, in high numbers, to count ſo many fields, the gages are generally ſingle. Where the ſtars happened to be uncommonly crouded, no more than half a field was counted, and even ſometimes only a quadrant; but then it was always done with the precaution of fixing on ſome row of ſtars that would point out the diviſion of the field,

ſo

ſo as to prevent any conſiderable miſtake. When five, ten, or more fields are gaged, the polar diſtance in the ſecond column of the table is that of the middle of the ſweep, which was generally from 2 to 2½ degrees in breadth; and, in gaging, a regular diſtribution of the fields, from the bottom of the ſweep to the top, was always ſtrictly attended to. The fifth column contains occaſional remarks relating to the gages.

I. Table of Star-Gages.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
0 1 41	78 47	9,9	10	
0 4 55	65 36	20,0	10	
0 7 54	74 13	11,3	10	Moſt of the ſtars extremely ſmall.
0 8 24	49 7	60	1	
0 9 52	113 17	4,1	10	* The gages marked with an aſteriſk
0 12 52	113 17	3,2	10	* are thoſe by which fig. 4. tab.
0 16 48	67 44	11,9	10	VIII. has been delineated.
0 21 52	113 17	3,9	10	*
0 22 21	87 10	5,9	10	
0 28 26	46 54	60	1	
0 31 38	46 54	40	1	
0 33 33	65 32	20,4	10	
0 34 22	56 38	20	1	
0 35 22	55 38	24	1	
0 36 39	76 32	11,3	10	
0 39 56	78 43	8,1	10	
0 40 29	48 43	60	½	
0 44 21	87 10	7,6	10	
0 46 22	69 51	11	10	
0 46 33	65 32	13	10	
0 48 42	58 47	40	1	
0 48 50	58 13	17	1	
0 53 18	67 41	9,8	10	A little hazy.
0 53 40	45 37	73	1	
0 54 10	75 16	13	1	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
0 55 10	73 16	14	1	
0 56 4	74 0	15	1	
0 57 52	113 17	3,8	10	*
0 59 10	74 25	14	1	
1 0 16	74 16	11,1	10	
1 1 10	74 5	11,2	10	
1 1 18	111 0	5,2	10	Very clear for this altitude.
1 2 52	52 0	28,1	10	Moſt of the ſtars very ſmall.
1 3 52	113 17	2,8	10	*
1 4 15	94 52	7,5	10	
1 4 33	65 32	11,0	10	
1 5 55	78 31	9,2	10	
1 7 27	45 23	58	1	
1 12 0	58 37	20	1	
1 12 48	60 19	13	1	
1 13 4	94 50	6,3	10	
1 15 51	48 40	30	1	
1 18 21	48 40	58	1	
1 23 21	48 40	44	1	
1 27 30	65 42	12,9	10	
1 31 21	87 7	5,8	10	
1 32 4	94 50	7,3	10	
1 33 10	100 8	6,4	10	
1 33 32	92 35	7,1	10	
1 34 52	60 8	17	1	
1 43 30	65 42	14,4	10	
1 45 24	69 43	7,1	10	
1 48 4	100 12	4,9	10	
1 54 24	76 28	12,1	10	
1 58 55	61 55	15,0	10	
2 4 28	87 5	6,4	10	
2 4 36	78 38	9,3	10	
2 7 12	94 56	7,8	10	
2 8 0	83 3	7,3	10	
2 10 4	100 12	4,3	10	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
2 11 30	65 45	14,8	10	
2 16 27	110 54	4,2	10	*
2 19 27	76 24	9,9	10	
2 22 17	45 31	82	1	
2 23 6	60 16	14	1	
2 23 19	113 8	4,2	10	*
2 24 6	58 30	15	1	
2 27 40	115 21	3,0	10	* The ſituation too low for great accuracy.
2 30 0	94 56	6	10	
2 31 23	76 22	13,8	10	
2 35 14	87 2	5,6	10	
2 38 0	94 56	6,6	10	
2 42 7	61 50	14,8	10	
2 47 32	74 3	11,1	10	Moſt of the ſtars exceedingly ſmall.
2 49 22	92 55	9,0	10	
2 49 30	110 55	6,1	10	*
2 50 0	94 56	6,8	10	
2 54 53	76 22	9,2	10	
2 59 56	81 10	6,1	10	
3 1 53	78 37	4,1	10	
3 1 56	81 10	5,1	10	
3 4 53	78 37	3,5	10	
3 10 20	100 2	6,8	10	
3 11 6	59 29	7,0	5	In a part of the heavens which looks pretty full of ſtars to the naked eye.
3 13 6	59 29	6,1	10	
3 15 6	59 29	9,4	10	
3 22 57	83 1	10,3	10	
3 23 21	92 49	10,1	10	
3 29 41	46 35	55	1	
3 35 0	62 1	15	1	About 15 ſtars generally in the field.
3 35 12	100 3	7,4	10	
3 36 1	113 3	4,9	10	*
3 42 49	46 10	54	1	
3 48 16	99 59	8,1	10	
3 55 11	74 2	11,0	10	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
4 1 24	92 48	13,8	10	
4 6 18	82 57	13,4	10	
4 8 31	114 55	4,2	10	*
4 12 41	69 33	15,3	10	And many more, extremely ſmall,
4 16 34	112 45	6,2	10	* ſuſpected.
4 26 34	112 45	8,8	10	*
4 27 11	70 41	25	1	
4 28 41	70 1	17	1	
4 29 5	69 24	30	1	
4 30 14	99 50	9,7	10	
4 31 19	67 33	15,6	10	
4 32 29	69 2	36	1	
4 33 31	114 55	8,1	10	*
4 42 14	86 27	19,9	10	
4 53 22	72 59	56	1	
4 57 45	83 22	38	1	
4 58 45	84 36	35	1	
5 1 16	69 23	34	1	
5 3 45	83 29	17,7	6	
5 10 52	69 22	74	1	
5 11 22	96 37	24	1	
5 17 22	96 15	8,9	8	
5 18 0	80 46	30	1	About 30 ſtars in the field, not very
5 21 7	92 52	19,1	10	exactly gaged.
5 24 12	66 5	36	1	
5 27 3	68 52	58	1	
5 27 48	110 40	17,7	10	*
5 33 4	76 10	65	1	
5 33 12	66 26	86	1	
5 33 17	114 59	13,5	10	*
5 34 45	70 33	50	1	
5 36 30	62 1	20—30		From 20 to 30 ſtars in the fields, not
5 37 4	74 26	140	½	very exactly gaged.
5 38 45	70 8	73	1	
5 41 12	66 43	60	1	

R.A.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
5 44 0	116 43	11,5	10	*
5 45 30	83 30	50	1	
5 47 34	112 34	19,3	10	*
5 48 30	62 1	30	1	About 30 ſtars in the field; not very
5 48 44	92 51	22,4	5	exactly gaged.
5 49 0	80 5	50	1	
5 52 14	93 14	44	1	
5 52 30	83 30	60	1	
5 53 0	80 5	110	1	
5 55 4	92 56	57	1	
5 56 40	70 27	73	1	
5 57 0	80 5	60	1	
5 57 37	110 33	19,6	10	*
5 58 51	88 36	90	1	
5 59 30	83 30	80	1	
6 0 23	86 38	24,1	10	
6 1 0	80 5	70	1	
6 4 0	80 5	90	1	
6 5 4	67 17	120	¼	Very unequally ſcattered.
6 6 14	96 16	52	1	
6 6 30	83 30	80	1	
6 6 30	80 5	70	1	
6 6 38	91 45	54	1	Like the reſt, or many ſuch fields.
6 6 40	68 24	56	1	
6 9 0	80 5	74	1	
6 9 34	113 35	26	1	*
6 11 0	62 1	30—40	1	Between.
6 11 0	80 5	63	1	The leaſt number of ſtars in the field I
6 11 34	112 5	33	1	* could find in this neighbourhood.
6 11 37	90 15		1	About 60 or 70 generally.
6 14 4	68 11	178	¼	
6 14 38	90 15	77	1	
6 17 45	62 1	50	1	
6 18 14	96 12	38	1	Very unequally ſcattered.
6 19 14	93 59	72	1	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
6 26 17	114 59	15,9	10	
6 27 14	94 36	132	½	*
6 27 32	70 23	50	1	
6 31 48	115 40	40	1	
6 34 44	92 25	94	1	
6 34 55	79 5	50	1	Generally about 50 ſtars.
6 36 0	94 56	62	1	Twilight.
6 37 15	75 5	70	1	Generally about 70 ſtars.
6 39 8	99 7	50	1	*
6 40 0	116 43	31,3	10	
6 43 25	79 5	67	1	
6 44 28	100 30	67	1	*
6 49 5	87 21	120	½	*
6 49 30	77 31	50	1	Many fields like this.
6 49 44	92 33	120	½	
6 51 8	98 33	78	1	*
6 52 0	116 21	48	1	
6 52 25	79 5	60	1	About 60 ſtars.
6 52 44	92 59	98	1	
6 54 9	111 11	45	1	*
6 57 8	100 1	34	1	*
6 57 38	98 50	83	1	*
6 58 39	112 48	81	1	*
7 0 25	79 5	70	1	
7 4 0	92 3	102	1	*
7 4 38	98 59	70	1	*
7 5 9	111 11	70	1	*
7 8 9	112 15	62	1	*
7 12 8	100 5	118	½	*
7 15 38	98 12	112	½	*
7 19 0	91 51	58	1	*
7 20 0	78 59	48	1	
7 25 9	111 21	168	¼	* One of the richeſt fields.
7 28 9	112 34	204	¼	* A field like the reſt.
7 33 3	115 28	86	1	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
7 41 9	113 26	108	½	*
7 53 4	86 39	28,3	10	*
8 1 4	111 15	80	1	*
8 3 4	113 31	66	1	
8 6 38	100 5	40	1	
8 7 38	99 3	45	1	*
8 11 8	99 25	24,2	10	*
8 12 34	112 15	52	1	*
8 22 4	111 30	35	1	*
8 31 4	112 1	33	1	
8 32 24	112 7	30	1	
8 35 4	112 17	24	1	
8 35 14	111 19	20	1	
8 40 4	111 11	22	1	*
8 45 4	113 22	13	1	
8 46 39	91 26	20,3	10	*
8 48 4	112 23	16,2	10	
8 57 25	66 20	8,3	10	*
9 5 38	91 22	13,8	10	*
9 10 4	115 17	14,0	10	
9 20 4	112 23	15,8	10	
9 20 40	99 12	11,1	10	
9 20 58	88 7	11,5	10	*
9 35 4	112 23	13,0	10	
9 38 4	115 17	10,1	10	
9 38 8	90 23	7,9	10	*
9 42 16	86 16	7,7	10	*
9 45 49	112 21	13,2	10	Strong twilight.
10 0 4	115 17	9,1	10	
10 16 8	88 8	7,2	10	*
10 19 32	91 14	6,5	10	
10 25 8	88 8	4,9	10	*
10 26 0	81 41	5,6	7	*
11 4 4	81 38	5,3	6	*
11 7 36	91 14	5,6	10	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
11 10 6	115 23	6,5	10	Twilight.
11 16 52	81 38	3,1	8	*
11 20 37	91 17	4,9	10	
11 53 43	81 39	6,0	5	*
12 5 6	78 57	2,2	13	*
12 30 40	79 3	3,4	11	*
12 46 51	81 40	4,6	13	*
12 48 19	79 4	3,9	13	*
12 53 45	101 45	9,3	10	Twilight.
12 57 8	99 56	8,1	10	Pretty ſtrong day-light.
13 1 19	79 4	3,8	12	*
13 17 27	101 45	8,6	10	Twilight.
13 22 49	100 1	8,4	10	Some day-light.
13 27 57	101 45	11,3	10	
13 31 10	75 55	5—6	1	* Generally about 5 or 6 ſtars in the field.
13 38 53	104 27	8,5	10	
13 48 49	100 1	9,2	10	Strong twilight.
13 51 27	101 45	10,0	10	
13 55 44	58 11	7,4	10	* Twilight.
13 57 53	104 27	12,3	10	Moſt very ſmall.
14 9 49	100 1	11,2	10	Twilight.
14 13 52	113 4	9,7	10	
14 14 57	101 45	8,8	10	
14 24 49	81 53	2,7	6	
14 29 45	100 5	13,3	10	
14 30 7	66 3	8,8	10	* All ſizes.
14 30 8	80 38	3,5	13	
14 33 22	58 7	8,9	10	* Chiefly ſmall.
14 33 52	113 4	10,3	10	
14 39 57	101 45	14,0	10	All ſizes.
14 40 36	64 47	6,4	10	
14 44 11	114 54	10,3	10	
14 49 52	113 4	12,8	10	
14 51 14	58 10	9,2	10	* Twilight.
14 52 58	60 41	4,4	10	* Strong Aurora borealis.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
14 53 7	66 15	9,0	10	Chiefly large.
14 55 36	64 47	6,6	10	Moſt very ſmall.
14 59 11	114 54	8,8	10	
15 2 42	62 48	8,3	10	
15 3 7	66 15	9,5	10	
15 4 36	64 47	5,0	10	
15 8 37	113 0	14,1	10	
15 8 45	93 5	9,4	12	Very ſmall.
15 13 42	62 48	8,9	10	
15 15 44	58 17	10,0	10	* Twilight.
15 19 48	60 40	4,9	10	* Strong Aurora borealis, ſo as to affect the gages.
15 20 0	75 52	9,5	4	
15 21 0	93 5	10,9	12	
15 26 7	81 53	11,0	5	
15 28 48	99 51	13,1	10	
15 29 7	66 15	10,6	10	All ſizes.
15 29 44	58 17	8,9	10	* Twilight.
15 32 0	75 51	6	6	
15 33 52	111 32	12,8	10	
15 35 0	75 51	6,5	6	
15 42 2	58 14	13,1	10	* Twilight.
15 42 3	116 56	18,6	10	
15 42 53	113 47	32,5	2	The ſtars too ſmall for the gage.
15 46 30	93 5	10,8	12	
15 48 37	113 0	17,1	10	
15 48 46	63 4	12,4	10	
15 49 52	111 32	18,1	10	The ſituation ſo low that it requires attention to ſee the ſtars.
15 50 20	114 55	9,2	10	
15 57 3	116 56	7,2	10	
16 0 2	58 14	12,2	10	* Twilight.
16 0 3	116 56	6,1	10	
16 0 12	114 57	1,6	10	
16 3 12	114 57	2,0	10	
16 4 0	75 43	13	6	All ſizes.
16 4 19	113 6	,5	10	Perfectly clear. See p. 256.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
16 4 46	63 4	12,0	10	Moſt ſmall.
16 4 52	99 57	14,6	10	Moon and twilight.
16 6 28	113 4	,7	10	Perfectly clear.
16 7 12	66 15	13,3	10	
16 8 6	115 1	3,8	6	
16 8 11	93 9	12,2	12	
16 8 16	116 48	11,6	10	
16 9 28	113 4	1,1	10	Perfectly clear. See p. 256.
16 11 28	113 4	1,4	10	The ſame.
16 13 28	113 4	1,8	10	*g* Serpentarii and 19 Scorpii viſible to
16 13 52	58 24	14,2	10	* Moſt ſmall. [the naked eye.
16 14 42	63 7	15,1	10	Moſt very ſmall.
16 15 37	80 40	9,7	12	All ſizes.
16 17 28	113 4	4,7	10	
16 20 51	81 57	13,8	6	
16 23 0	73 43	24	1	
16 23 28	113 4	13,6	10	
16 24 11	93 9	13,6	12	Require attention to be ſeen.
16 25 7	80 40	14,6	13	
16 27 32	68 23	21,6	10	Twilight.
16 29 16	116 48	50,4	10	
16 30 37	80 40	34	1	
16 31 12	66 15	18,4	10	Strong twilight.
16 32 28	113 4	20,3	10	Moſt extremely ſmall.
16 32 52	58 24	15,6	10	* Moſt ſmall.
16 35 42	63 7	16,5	10	*
16 35 48	93 15	18,6	12	All ſizes.
16 38 12	66 15	20,1	10	Strong twilight.
16 38 45	107 57	19,9	10	Strong twilight.
16 40 51	113 14	41,1	8	
16 45 32	68 23	19,0	4	Hazy.
16 51 45	107 57	29,8	10	
16 52 22	66 26	16,6	10	Day-light pretty ſtrong.
16 55 42	63 7	26,6	10	* Strong twilight.
17 1 34	58 11	18,8	10	* Strong day light.

R.A.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
17 3 22	66 26	35	1	* Day-light too ſtrong for gaging.
17 6 36	98 38	13,7	10	Moſt ſmall, and more ſuſpected.
17 9 30	116 55	7,6	10	
17 9 32	68 23	32,3	10	
17 11 10	66 26	38	1	* Day-light pretty ſtrong.
17 13 24	63 21	32,8	10	* Strong day-light.
17 17 36	111 47	15,3	10	Moon and day-light.
17 25 7	108 5	23	10	
17 27 29	116 48	25	1	
17 28 32	68 23	42,2	5	* Twilight.
17 30 29	116 48	42	1	
17 33 29	116 48	52	1	Day-light very ſtrong.
17 34 36	98 38	18,5	10	Very ſtrong twilight.
17 39 34	120 0	84	1	Moſt large.
17 40 41	114 52	77	1	Day-light very ſtrong.
17 41 29	116 48	82	1	Day-light very ſtrong.
17 43 45	105 3	80	1	Flying clouds.
17 48 0	61 18	25,6	5	Moſt large.
17 50 4	56 16	27,2	10	Twilight.
17 50 7	108 5	59	1	Like the reſt in this part of the heaven.
17 52 7	108 5	118	1	Many ſuch fields juſt by.
17 52 17	98 43	7,6	10	
17 52 30	62 12	40	1	Moſt large.
17 52 32	68 19	54	1	* Strong day-light.
17 55 7	108 5	232	½	
17 55 15	106 6	112	1	Many ſuch fields.
17 55 38	112 54	112	½	
17 57 30	60 28	38	1	Moſt large.
17 58 37	103 24	35	1	
17 58 41	118 57	64	1	
17 58 49	122 17	17	1	
17 59 1	108 8	320	½	
17 59 19	104 24	68	1	
18 0 13	122 11	27	1	
18 3 49	120 42	19	1	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S	D. M.			
18 5 17	98 47	65	1	
18 6 37	90 36	9,4	10	Too ſoon for gaging, not having been
18 7 4	62 14	40	1	Moſt large. long enough out
18 7 4	56 16	38,2	5	in the dark.
18 7 37	103 25	88,0	3	
18 10 7	120 58	20	1	
18 10 52	61 8	78	1	Chiefly large.
18 11 49	104 6	170	½	
18 13 37	104 16	238	½	
18 13 52	93 11	2,0	7	
18 14 46	56 20	48	1	
18 15 28	92 42	3,4	7	
18 16 52	92 42	8,9	7	
18 18 40	92 42	13,8	7	
18 19 37	102 34	9,5	2	
18 20 7	103 18	19	1	
18 20 46	92 42	25,8	6	
18 21 1	103 55	22	1	
18 21 12	90 41	8,6	10	
18 21 31	103 36	24	1	
18 22 4	62 7	48	1	Large and ſmall.
18 22 4	56 16	39,6	5	
18 22 19	104 6	14	1	
18 22 37	103 45	30	1	
18 24 3	115 10	35	1	
18 24 4	109 35	35	1	Twilight.
18 24 7	102 31	30	1	
18 24 10	92 59	88	1	
18 24 43	103 39	25	1	
18 25 37	102 34	39	1	
18 26 17	98 3	111	1	
18 26 25	103 57	60	1	
18 26 47	97 43	250	1	
18 27 1	120 58	30	1	
18 27 55	120 44	32	1	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
18 28 7	102 51	13	1	Extremely ſmall.
18 28 8	91 44	39	1	Moſt ſmall.
18 28 25	103 9	20	1	Extremely ſmall.
18 28 37	122 25	12	1	
18 29 25	103 24	20	1	Extremely ſmall.
18 29 47	97 50	150	1	
18 29 49	121 39	24	1	
18 30 34	57 18	62	1	
18 31 10	92 42	13,7	7	
18 31 10	108 53	74	1	Twilight.
18 31 13	103 19	112	1	All ſizes.
18 31 17	97 53	188	½	Many more ſuſpected.
18 31 34	62 34	76	1	* Large and ſmall.
18 31 49	121 39	19,3	10	
18 33 4	108 43	88	1	Twilight.
18 33 7	103 53	146	½	
18 34 5	98 34	130	1	
18 34 47	71 53	78	1	*
18 34 58	60 41	80	1	Large and ſmall.
18 36 34	110 12	83	1	Twilight.
18 36 34	91 37	176	¼	
18 36 47	72 28	224	½	*
18 37 34	93 29	5	1	
18 38 1	104 14	118	½	
18 39 40	93 52	116	¼	
18 40 28	92 47	10	1	
18 40 47	71 48	236	¼	*
18 41 22	91 37	156	¼	
18 42 49	121 39	15,2	10	Very clear for this altitude.
18 43 17	72 8	368	¼	*
18 43 33	119 21	21	1	
18 44 34	112 43	53	1	
18 44 34	60 34	84	1	All ſizes.
18 47 32	91 14	328	¼	
18 48 4	110 12	83		

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
18 50 16	60 55	136	½	Many of them ſmall.
18 51 4	57 26	84	1	
18 51 32	108 26	36,8	5	Strong twilight.
18 52 49	115 30	26,2	5	
18 54 4	57 18	93	1	
18 54 8	91 14	328	¼	
18 54 55	104 23	180	½	
18 55 4	108 41	80	1	
18 55 16	62 31	206	½	
18 59 8	91 14	328	¼	
18 59 26	72 37	40	1	Too ſoon for gaging.
19 1 2	71 40	75	1	
19 1 34	56 47	127	1	Moonlight.
19 2 29	74 53	204	¼	* Twilight.
19 2 37	103 16	160	½	
19 2 49	121 39	14,1	10	
19 3 34	55 56	146	½	☽
19 6 34	61 8	196	½	And many ſmall beſides.
19 7 34	56 56	130	½	☽
19 7 52	57 59	116	½	
19 8 38	92 8	120	½	
19 9 37	109 1	60	1	
19 9 40	56 51	130	1	☽
19 12 59	75 21	58	1	*
19 13 50	59 59	256	¼	
19 13 52	59 29	158	½	
19 14 2	72 15	60	1	*
19 14 4	61 21	279	⅓	Too crowded for accuracy.
19 14 55	103 36	64	1	Changeable focus.
19 15 40	55 26	160	1	☽ bright.
19 16 50	60 43	296	¼	
19 16 59	73 23	56	1	*
19 17 44	108 12	50	1	
19 18 23	78 9	196	¼	*
19 18 28	61 21	279	⅓	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
19 19 52	57 14	180	1/2	
19 19 56	108 12	55	1	
19 20 51	60 55	384	1/4	
19 21 1	78 47	472	1/4	*
19 21 34	55 17	208	1/2	☽ bright.
19 22 27	62 29	320	1/4	
19 24 36	56 49	224	1/4	
19 24 49	104 24	36	1	Changeable focus.
19 24 50	60 43	296	1/4	
19 24 53	113 51	18,3	10	
19 25 4	57 9	190	1/2	☽ bright.
19 25 16	64 18	280	1/4	
19 25 22	59 36	340	1/4	
19 25 37	103 50	55	1	Changeable focus.
19 27 36	72 34	424	1/4	* Too ſmall and too crowded to be cer-
19 27 44	61 8	240	1/3	[tain of the number.
19 28 1	103 30	45	1	Changeable focus.
19 28 6	56 49	288	1/4	
19 28 52	59 26	344	1/4	
19 28 52	56 47	186	1/2	☽ very bright.
19 29 46	65 10	34	1	
19 30 36	74 33	588	1/4	*
19 30 36	54 53	312	1/4	
19 31 33	92 34	62,2	5	*
19 32 9	109 44	23,8	10	
19 32 15	62 35	296	1/4	
19 33 4	55 34	212	1/2	☽
19 33 7	103 12	50	1	Changeable focus.
19 33 14	61 8	240	1/3	
19 33 20	58 59	232	1/4	
19 34 51	115 44	14,1	10	
19 35 34	63 19	256	1/4	
19 36 6	54 57	384	1/4	
19 36 37	102 31	68	1	Changeable focus.
19 36 50	60 35	296	1/4	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
19 40 33	63 0	296	¼	
19 40 46	59 12	192	¼	
19 40 48	74 33	588	¼	*
19 42 33	73 14	352	¼	*
19 43 30	57 23	130	½	D
19 43 56	64 27	124	½	Moſt large.
19 45 36	77 58	140	½	* Faint D.
19 45 37	103 3	50	1	
19 46 21	73 14	352	¼	*
19 46 51	115 44	12,8	10	Strong twilight.
19 47 8	60 35	312	¼	
19 47 18	109 46	20,9	10	
19 47 22	57 38	312	¼	Very unequally ſcattered.
19 49 6	57 13	268	¼	
19 49 48	56 51	120	½	D
19 50 5	92 39	39,2	5	* Moſt ſmall.
19 51 37	62 37	51	1	
19 52 0	57 15	220	½	D
19 53 1	60 36	80	1	
19 53 28	63 40	52	½	
19 53 40	54 59	306	¼	
19 53 49	121 39	7,7	10	
19 54 0	55 12	160	½	D
19 54 12	78 3	120	½	* Faint D.
19 54 22	59 58	136	¼	
19 55 7	62 41	48	1	
19 56 19	60 44	112	½	
19 56 22	57 17	192	¼	
19 57 19	62 34	45	1	
19 57 40	58 29	104	½	
19 59 49	62 37	41	1	
20 0 21	79 3	56	1	* Strong D.
20 0 24	55 12	184	½	D
20 0 25	60 33	80	1	Moſt of the ſtars extremely ſmall.
20 0 51	115 44	12,2	10	Twilight.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
20 1 39	79 34	68	1	* Strong D.
20 5 26	56 34	46	1	D
20 5 27	72 56	280	1/4	
20 6 23	107 27	22,6	10	
20 6 43	62 32	75	1	Many ſmall.
20 8 26	56 27	47,4	5	D
20 8 27	72 56	280	1/4	
20 8 58	103 37	38	1	
20 9 6	109 40	24,2	5	
20 9 52	102 48	31	1	
20 12 22	58 14	76	1/2	
20 17 20	76 12	184	1/4	Some twilight.
20 18 51	115 44	10,6	10	Twilight.
20 20 58	61 27	88	1	
20 21 36	71 28	104	1/2	Hazy.
20 22 56	56 27	66	1	D
20 22 58	103 26	20	1	
20 24 51	115 44	9,3	10	Twilight.
20 25 58	103 26	22,8	10	Changeable focus.
20 25 59	67 27	248	1/4	
20 26 1	92 44	30,8	5	*
20 26 46	109 37	16,7	10	Not clear.
20 26 49	121 39	7,7	10	A little hazy.
20 27 33	96 7	39	1	Moſt ſmall.
20 34 51	115 44	9,5	10	D
20 35 53	61 20	142	1/2	
20 37 18	58 28	108	1/2	
20 37 34	97 6	26,6	10	*
20 38 1	92 44	28,2	5	*
20 39 42	66 37	78	1/2	
20 40 22	56 21	192	1/4	
20 41 11	67 54	108	1/2	
20 41 56	74 33	116	1/2	
20 42 59	62 14	112	1/2	
20 43 1	70 29	76	1	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
20 43 30	54 47	260	$\frac{1}{4}$	Moſt of the ſtars of the ſame ſize.
20 44 59	70 6	80	1	
20 47 13	60 46	120	$\frac{1}{2}$	
20 49 1	92 44	27,0	5	*
20 49 10	57 11	248	$\frac{1}{4}$	Moſt of a ſize.
20 50 59	103 26	17,2	3	
20 51 23	68 30	70	1	
20 53 29	103 26	17,4	5	
20 54 1	107 47	10,3	10	
20 56 59	103 26	14,9	10	Moſt extremely ſmall.
20 57 55	61 25	64	1	Twilight.
20 59 1	92 44	21,4	5	*
21 1 6	96 43	40	1	* Moſt ſmall.
21 3 29	66 39	80	$\frac{1}{2}$	
21 3 53	73 9	55	1	
21 6 13	69 23	40	1	A little hazy.
21 6 55	103 32	11,1	10	
21 7 49	109 45	12,8	10	
21 7 59	64 58	110	$\frac{1}{2}$	
21 9 25	61 36	75	1	Strong twilight.
21 10 13	60 39	70	1	Strong twilight.
21 11 17	73 18	50	1	
21 11 42	96 13	25	1	*
21 12 1	92 44	16,4	5	
21 15 3	109 56	15,3	10	
21 16 43	59 7	76	$\frac{1}{2}$	
21 18 54	57 20	50	1	
21 20 18	96 43	24	1	*
21 21 0	107 49	8,1	10	
21 22 14	76 33	30,0	5	
21 25 31	92 44	8,0	5	
21 29 12	83 11	21,6	5	
21 30 58	78 57	18,9	10	
21 32 10	57 14	25	1	
21 33 1	92 44	15,4	5	Strong twilight.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
21 34 55	97 17	13,6	10	*
21 36 38	65 55	42	1	
21 38 20	65 38	60	1	
21 39 55	96 17	18	1	*
21 41 52	58 42	44	1	
21 43 22	109 55	11,5	10	*
21 45 4	59 39	52	1	
21 48 22	59 30	29	1	
21 51 52	58 56	61	1	
21 51 55	97 17	11,5	10	*
21 54 22	109 55	12,8	10	*
21 57 49	59 37	60	½	
21 58 4	75 7	33	1	
21 58 19	59 6	40	½	
21 58 43	58 34	32,6	5	☽
21 58 49	58 20	34	1	
22 2 25	60 9	42,6	5	
22 2 52	109 55	7,4	10	*
22 3 56	71 48	25,1	10	
22 7 22	109 55	8,9	10	*
22 10 28	75 2	26	1	
22 11 32	97 14	10,7	10	* Twilight.
22 11 35	65 48	26,6	5	
22 18 32	97 14	9,1	10	* Twilight.
22 20 35	109 58	8,3	10	*
22 20 55	78 54	11,7	10	Bright ☽.
22 27 41	95 4	8,1	10	
22 30 35	109 58	5,0	10	*
22 31 28	73 59	17,3	10	
22 33 6	76 52	16,5	10	
22 34 40	61 56	20,1	10	
22 35 35	109 58	7,1	10	*
22 36 49	71 57	18,5	10	
22 39 41	82 5	19	1	
22 40 5	65 48	21,3	10	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
22 43 55	60 9	26,7	10	Faint)
22 45 3	80 47	13,2	10	
22 45 30	58 38	17,2	10	)
22 48 49	71 57	13,4	10	
22 52 9	78 43	8,2	10	)
22 52 41	95 4	8,9	10	
22 55 40	71 54	11,6	10	
22 56 55	67 53	12,1	10	
22 58 19	78 42	9,2	10	)
23 0 27	113 12	4,4	10	
23 0 30	58 38	18,7	10	)
23 2 59	65 50	21,3	10	
23 5 35	109 58	7,3	10	)
23 8 52	95 1	7,5	10	Moſt extremely ſmall.
23 10 4	64 55	26	1	
23 11 40	61 48	21,1	10	
23 12 40	71 54	11,9	10	
23 17 50	81 0	9,7	10	
23 23 58	69 48	12,1	10	
23 25 32	113 12	3,1	10	*
23 32 2	69 51	9,5	10	
23 33 20	79 45	10	1	
23 43 2	69 51	10,9	10	
23 44 47	45 24	50	1	
23 46 52	113 17	4,2	10	*
23 46 55	65 36	15,3	10	
23 59 21	87 10	5,6	10	
23 59 56	95 4	7,8	10	

PRO-

PROBLEM.

The ſtars being ſuppoſed to be nearly equally ſcattered, and their number, in a field of view of a known angular diameter, being given, to determine the length of the viſual ray.

Here, the arrangement of the ſtars not being fixed upon, we muſt endeavour to find which way they may be placed ſo as to fill a given ſpace moſt equally. Suppoſe a rectangular cone cut into fruſtula by many equidiſtant planes perpendicular to the axis; then, if one ſtar be placed at the vertex, and another in the axis at the firſt interſection, ſix ſtars may be ſet around it ſo as to be equally diſtant from one another and from the central ſtar. Theſe poſitions being carried on in the ſame manner, we ſhall have every ſtar within the cone ſurrounded by eight others, at an equal diſtance from that ſtar taken as a center. Fig. 1. (tab. VIII.) contains four ſections of ſuch a cone diſtinguiſhed by alternate ſhades, which will be ſufficient to explain what ſort of arrangement I would point out.

The ſeries of the number of ſtars contained in the ſeveral ſections will be 1 . 7 . 19 . 37 . 61 . 91 . &c. which continued to n terms, the ſum of it, by the differential method, will be $na+n\cdot\frac{n-1}{2}d'+n\cdot\frac{n-1}{2}\cdot\frac{n-2}{3}d''$, &c.: where a is the firſt term d', d'', d''', &c. the 1ſt, 2d, and 3d differences. Then, ſince $a=1$, $d'=6$, $d''=6$, $d'''=0$, the ſum of the ſeries will be n^3. Let S be the given number of ſtars; 1, the diameter of the baſe of the field of view; and B, the diameter of the baſe of the great rectangular cone; and, by trigonometry, we ſhall have $B=\frac{\text{Radius.}}{\text{Tang. }\frac{1}{2}\text{ field}}$. Now, ſince the

field of view of a teleſcope is a cone, we ſhall have its ſolidity to that of the great cone of ſtars, formed by the above conſtruction as the ſquare of the diameter of the baſe of the field of view, to the ſquare of the diameter of the baſe of the great cone, the height of both being the ſame; and the ſtars in each cone being in the ratio of the ſolidity, as being equally ſcattered (*b*), we have $n = \sqrt[3]{B^2S}$. And the length of the viſual ray $= n - 1$, which was to be determined.

(*b*) We ought to remark, that the periphery and baſe of the cone of the field of view, in gaging, would in all probability ſeldom fall exactly on ſuch ſtars as would produce a perfect equality of ſituation between the ſtars contained in the ſmall and the great cone; and that, conſequently, the ſolution of this problem, where we ſuppoſe the ſtars of one cone to be to thoſe of the other in the ratio of the ſolidity on account of their being equally ſcattered, will not be ſtrictly true. But it ſhould be remembered, that in ſmall numbers, where the different terminations of the fields would moſt affect this ſolution, the ſtars in view have always been aſcertained from gages that were often repeated, and each of which conſiſted of no leſs than ten fields ſucceſſively taken, ſo that the different deviations at the periphery and baſe of the cone would certainly compenſate each other ſufficiently for the purpoſe of this calculation. And that, on the other hand, in high gages, which could not have the advantage of being ſo often repeated, theſe deviations would bear a much ſmaller proportion to the great number of ſtars in a field of view; and therefore, on this account, ſuch gages may very juſtly be admitted in a ſolution where practical truth rather than mathematical preciſion is the end we have in view. It is moreover not to be ſuppoſed that we imagine the ſtars to be actually arranged in this regular manner, and, returning therefore to our general hypotheſis of their being equally ſcattered, any one field of view promiſcuouſly taken may, in this general ſenſe, be ſuppoſed to contain a due proportion of them; ſo that the principle on which this ſolution is founded may therefore be ſaid to be even more rigorouſly true than we have occaſion to inſiſt upon in an argument of this kind.

The ſame otherwiſe.

If a different arrangement of the ſtars ſhould be ſelected, ſuch as that in fig. 2. where one ſtar is at the vertex of a cone; three in the circumference of the firſt ſection, at an equal diſtance from the vertex and from each other; ſix in the circumference of the next ſection, with one in the axis or center; and ſo on, always placing three ſtars in a lower ſection in ſuch a manner as to form an equilateral pyramid with one above them: then we ſhall have every ſtar, which is ſufficiently within the cone, ſurrounded by twelve others at an equal diſtance from the central ſtar and from each other. And by the differential method, the ſum of the two ſeries equally continued, into which this cone may be reſolved, will be $2n^3 + 1\frac{1}{2}n^2 + \frac{1}{2}n$; where n ſtands for the number of terms in each ſeries. To find the angle which a line $v\,x$, paſſing from the vertex v over the ſtars v, n, h, l, &c. to x, at the outſide of the cone, makes with the axis; we have, by conſtruction, $v\,s$ in fig. 3. repreſenting the planes of the firſt and ſecond ſections $= 2 \times \text{cof.}30^\circ = \varphi$, to the radius $p\,s$, of the firſt ſection $= 1$. Hence it will be $\sqrt{\varphi^2 - 1} = v\,p = \frac{1}{2}\,v\,m$; or $v\,m = 2\sqrt{\varphi^2 - 1}$: and, by trigonometry, $\frac{R\,\varphi}{2\sqrt{\varphi^2 - 1}} = T$. Where T is the tangent of the required angle to the radius R (*c*); and putting $t =$ tangent of

(*c*) In finding this angle we have ſuppoſed the cone to be generated by a revolving rectangular triangle of which the line $v\,x$, fig. 2. is the hypotenuſe; but the ſtars in the ſecond ſeries will occaſion the cone to be contained under a waving ſurface, wherefore the above ſuppoſition of the generation of the cone is not ſtrictly true; but then theſe waves are ſo inconſiderable, that, for the preſent purpoſe, they may ſafely be neglected in this calculation.

half the given field of view, it will be $\frac{T}{t} = B$, the baſe of the cone. And $\frac{\sqrt{\varphi^2 - 1}}{\varphi} = d$, will be an expreſſion for vp, in terms of vs, which is the mutual diſtance of the ſcattered ſtars. Then having $\frac{B^2 S}{2} = n^3 + \frac{3}{4} n^2 + \frac{1}{4} n$, we may find n; whence $2dn - d$, the viſual ray, will be obtained.

The reſult of this arrangement gives a ſhorter ray than that of the former; but ſince the difference is not ſo conſiderable as very materially to affect the concluſions, I ſhall, on account of the greater convenience, make uſe of the firſt.

We inhabit the planet of a ſtar belonging to a Compound Nebula of the third form.

I ſhall now proceed to ſhew that the ſtupendous ſidereal ſyſtem we inhabit, this extenſive ſtratum and its ſecondary branch, conſiſting of many millions of ſtars, is, in all probability, *a detached Nebula.* In order to go upon grounds that ſeem to me to be capable of great certainty, they being no leſs than an actual ſurvey of the boundaries of our ſidereal ſyſtem, which I have plainly perceived, as far as I have yet gone round it, every where terminated, and in moſt places very narrowly too, it will be proper to ſhew the length of my ſounding line, if I may ſo call it, that it may appear whether it was ſufficiently long for the purpoſe.

In the moſt crowded part of the milky way I have had fields of view that contained no leſs than 588 ſtars (*d*), and theſe were continued for many minutes, ſo that in one quarter of an hour's time there paſſed no leſs than 116000 ſtars through the field of

(*d*) See the table of Gages, p. 235.

view

view of my teleſcope (*e*). Now, if we compute the length of the viſual ray by putting S = 588, and the diameter of the field of view fifteen minutes, we ſhall find $n = \sqrt[2]{B^2 S} = 498$; ſo that it appears the length of what I have called my ſounding line, or $n - 1$, was probably not leſs than 497 times the diſtance of Sirius from the ſun. The ſame gage calculated by the ſecond arrangement of ſtars gives $\sqrt{\varphi^2 - 1} = 1.41421$; $\frac{R\varphi}{2.\sqrt{\varphi^2 - 1}} =$ tangent of 31° 28′ 55″,77; $\frac{T}{t} = B = 280,69$; $\frac{\sqrt{\varphi^2 - 1}}{\varphi} = d =$,81649; $\frac{B^2 S}{2} = 23163409,7 = n^3 + \frac{3}{4} n^2 + \frac{1}{4} n$; where $n = 284,8$ nearly; and $2dn - 1 = 464$, the viſual ray.

It may ſeem inaccurate that we ſhould found an argument on the ſtars being equally ſcattered, when in all probability there may not be two of them in the heavens, whoſe mutual diſtance ſhall be equal to that of any other two given ſtars; but it ſhould be conſidered, that when we take all the ſtars collectively there will be a mean diſtance which may be aſſumed as the general one; and an argument founded on ſuch a ſuppoſition will have in its favour the greateſt probability of not being far ſhort of truth. What will render the ſuppoſition of an equal diſtribution of the ſtars, with regard to the gages, ſtill leſs expoſed to objections is, that whenever the ſtars happened either to be uncommonly crowded or deficient in number, ſo as very ſud-

(*e*) The breadth of my ſweep was 2° 26′, to which muſt be added 15′ for two ſemi-diameters of the field. Then, putting 161 = *a*, the number of fields in 15 minutes of time; ,7854 = *b*, the proportion of a circle to 1, its circumſcribed ſquare; φ = fine of 74° 22′, the polar diſtance of the middle of the ſweep reduced to the preſent time; and 588 = S, the number of ſtars in a field of view, we have $\frac{a\varphi S}{b} = 116076$ ſtars.

denly

denly to paſs over from one extreme to the other, the gages were reduced to other forms, ſuch as the border-gage, the diſtance-gage, &c. which terms, and the uſe of ſuch gages, I ſhall hereafter find an opportunity of explaining. And none of thoſe kinds of gages have been admitted in this table, which conſiſts only of ſuch as have been taken in places where the ſtars apparently ſeemed to be, in general, pretty evenly ſcattered; and to increaſe and decreaſe in number by a certain gradual progreſſion. Nor has any part of the heavens containing a cluſter of ſtars been put in the gages; and here I muſt obſerve, that the difference between a crowded place and a cluſter may eaſily be perceived by the arrangement as well as the ſize and mutual diſtance of the ſtars: for in a cluſter they are generally not only reſembling each other pretty nearly in ſize, but a certain uniformity of diſtance alſo takes place; they are more and more accumulated towards the center, and put on all the appearances which we ſhould naturally expect from a number of them collected into a group at a certain diſtance from us. On the other hand, the rich parts of the milky way, as well as thoſe in the diſtant broad part of the ſtratum, conſiſt of a mixture of ſtars of all poſſible ſizes, that are ſeemingly placed without any particular apparent order. Perhaps we might recollect, that a greater condenſation towards the center of our ſyſtem than towards the borders of it ſhould be taken into conſideration; but, with a nebula of the third form, containing ſuch various and extenſive combinations, as I have found to take place in ours, this circumſtance, which in one of the firſt form would be of conſiderable moment, may, I think, be ſafely neglected. However, I would not be underſtood to lay a greater ſtreſs on theſe and the following calculations than the principles on which they are founded will permit; and if here-

after

after we ſhall find reaſon, from experience and obſervation, to believe that there are parts of our ſyſtem where the ſtars are not ſcattered in the manner here ſuppoſed, we ought then to make proper exceptions.

But to return: if ſome other high gage be ſelected from the table, ſuch as 472 or 344, the length of the viſual ray will be found 461 and 415. And although, in conſequence of what has been ſaid, a certain degree of doubt may be left about the arrangement and ſcattering of the ſtars, yet when I recollect, that in thoſe parts of the milky way where theſe high gages were taken, the ſtars were neither ſo ſmall, nor ſo crowded, as they muſt have been on a ſuppoſition of a much farther continuance of them, when certainly a milky or nebulous appearance muſt have come on, I need not fear to have over-rated the extent of my viſual ray. And indeed every thing that can be ſaid to ſhorten it will only contract the limits of our nebula, as it has in moſt places been of ſufficient length to go far beyond the bounds of it. Thus, in the ſides of the ſtratum oppoſite to our ſituation in it, where the gages often run below 5, our nebula cannot extend to 100 times the diſtance of Sirius; and the ſame teleſcope, which could ſhew 588 ſtars in a field of view of 15 minutes, muſt certainly have preſented me alſo with the ſtars in theſe ſituations as well as the former, had they been there. If we ſhould anſwer this by obſerving that they might be at too great a diſtance to be perceived, it will be allowing that there muſt at leaſt be a vacancy amounting to the length of a viſual ray not ſhort of 400 times the diſtance of Sirius; and this is amply ſufficient to make our nebula a detached one. It is true, that it would not be conſiſtent confidently to affirm that we were on an iſland unleſs we had actually found ourſelves every where bounded by the ocean,

ocean, and therefore I ſhall go no farther than the gages will authoriſe; but conſidering the little depth of the ſtratum in all thoſe places which have been actually gaged, to which muſt be added all the intermediate parts that have been viewed and found to be much like the reſt, there is but little room to expect a connection between our nebula and any of the neighbouring ones. I ought alſo to add, that a teleſcope with a much larger aperture than my preſent one, graſping together a greater quantity of light, and thereby enabling us to ſee farther into ſpace, will be the ſureſt means of compleating and eſtabliſhing the arguments that have been uſed: for if our nebula is not abſolutely a detached one, I am firmly perſuaded, that an inſtrument may be made large enough to diſcover the places where the ſtars continue onwards. A very bright milky nebuloſity muſt there undoubtedly come on, ſince the ſtars in a field of view will increaſe in the ratio of n^3, greater than that of the cube of the viſual ray. Thus, if 588 ſtars in a given field of view are to be ſeen by a ray of 497 times the diſtance of Sirius; when this is lengthened to 1000, which is but little more than double the former, the number of ſtars in the ſame field of view will be no leſs than 4774: for when the viſual ray r is given, the number S of ſtars will be $=\frac{n^3}{B^2}$; where $n=r+1$; and a teleſcope with a three-fold power of extending into ſpace, or with a ray of 1500, which, I think, may eaſily be conſtructed, will give us 16096 ſtars. Now, theſe would not be ſo cloſe but that a good power applied to ſuch an inſtrument might eaſily diſtinguiſh them; for they need not, if arranged in regular ſquares, approach nearer to each other than $6'',27$; but what would produce the milky nebuloſity which I have mentioned is the numberleſs ſtars beyond them, which in one

reſpect

reſpect the viſual ray might alſo be ſaid to reach. To make this appear we muſt return to the naked eye, which, as we have before eſtimated, can only ſee the ſtars of the ſeventh magnitude ſo as to diſtinguiſh them; but it is nevertheleſs very evident that the united luſtre of millions of ſtars, ſuch as I ſuppoſe the nebula in Andromeda to be, will reach our ſight in the ſhape of a very ſmall, faint nebuloſity; ſince the nebula of which I ſpeak may eaſily be ſeen in a fine evening. In the ſame manner my preſent teleſcope, as I have argued, has not only a viſual ray that will reach the ſtars at 497 times the diſtance of Sirius ſo as to diſtinguiſh them (and probably much farther), but alſo a power of ſhewing the united luſtre of the accumulated ſtars that compoſe a milky nebuloſity, at a diſtance far exceeding the former limits; ſo that from theſe conſiderations it appears again highly probable, that my preſent teleſcope, not ſhewing ſuch a nebuloſity in the milky way, goes already far beyond its extent: and conſequently, much more would an inſtrument, ſuch as I have mentioned, remove all doubt on the ſubject, both by ſhewing the ſtars in the continuation of the ſtratum, and by expoſing a very ſtrong milky nebuloſity beyond them, that could no longer be miſtaken for the dark ground of the heavens.

To theſe arguments, which reſt on the firm baſis of a ſeries of obſervation, we may add the following conſiderations drawn from analogy. Among the great number of nebulæ which I have now already ſeen, amounting to more than 900, there are many which in all probability are equally extenſive with that which we inhabit; and yet they are all ſeparated from each other by very conſiderable intervals. Some indeed there are that ſeem to be double and treble; and though with moſt of theſe it may be, that they are at a very great diſtance from each

other, yet we allow that ſome ſuch conjunctions really are to be found; nor is this what we mean to exclude. But then theſe compound or double nebulæ, which are thoſe of the third and fourth forms, ſtill make a detached link in the great chain. It is alſo to be ſuppoſed, that there may ſtill be ſome thinly ſcattered ſolitary ſtars between the large interſtices of nebulæ, which, being ſituated ſo as to be nearly equally attracted by the ſeveral cluſters when they were forming, remain unaſſociated. And though we cannot expect to ſee theſe ſtars, on account of their vaſt diſtance, yet we may well preſume, that their number cannot be very conſiderable in compariſon to thoſe that are already drawn into ſyſtems; which conjecture is alſo abundantly confirmed in ſituations where the nebulæ are near enough to have their ſtars viſible; for they are all inſulated, and generally to be ſeen upon a very clear and pure ground, without any ſtar near them that might be ſuppoſed to belong to them. And though I have often ſeen them in beds of ſtars, yet from the ſize of theſe latter we may be certain, that they were much nearer to us than thoſe nebulæ, and belonged undoubtedly to our own ſyſtem.

Uſe of the gages.

A delineation of our nebula, by an application of the gages in the manner which has been propoſed to be done in my former paper, may now be attempted, and the following table is calculated for this purpoſe. It gives us the length of the viſual ray for any number of ſtars in the field of view contained in the third column of the foregoing table of gages from $\frac{1}{10}$ to 100000. If the number required is not to be found in the firſt

column of this table, a proportional mean may be taken between the two neareſt rays in the ſecond column, without any material error, except in the few laſt numbers. The calculations of reſolvable and milky nebuloſity, at the end of the table, are founded, the firſt, on a ſuppoſition of the ſtars being ſo crowded as to have only a ſquare ſecond of ſpace allowed them; the next aſſigning them only half a ſecond ſquare. However, we ſhould conſider that in all probability a very different accumulation of ſtars may take place in different nebulæ; by which means ſome of them may aſſume the milky appearance, though not near ſo far removed from us; while cluſters of ſtars alſo may become reſolvable nebulæ from the ſame cauſe. The diſtinctneſs of the inſtrument is here alſo concerned; and as teleſcopes with large apertures are not eaſily brought to a good figure, nebulous appearances of both ſorts may probably come on much before the diſtance annexed to them in the table.

TABLE II.

Stars in the field	Visual ray.	Stars	Ray.	Stars.	Ray.	Stars.	Ray.	Stars.	Ray.
		31	186	71	245	210	352	700	527
0,1	27	32	188	72	246	220	358	800	551
0,2	34	33	190	73	247	230	363	900	573
0,3	39	34	192	74	249	240	368	1000	593
0,4	43	35	193	75	250	250	374	10000	1280
0,5	46	36	195	76	251	260	378	100000	2758
0,6	49	37	197	77	252	270	383		
0,7	52	38	199	78	253	280	388		
0,8	54	39	201	79	254	290	393		
0,9	56	40	202	80	255	300	397		
1	58	41	204	81	256	310	401		
2	74	42	206	82	257	320	406	636175	
3	85	43	207	83	258	330	410	or	5112
4	93	44	209	84	259	340	414	resolvable	
5	101	45	210	85	260	350	418	nebulosity	
6	107	46	212	86	261	360	422		
7	113	47	214	87	262	370	426		
8	118	48	215	88	263	380	430		
9	123	49	217	89	264	390	433		
10	127	50	218	90	265	400	437		
11	131	51	219	91	266	410	441		
12	135	52	221	92	267	420	444	2544700	
13	139	53	222	93	268	430	448	or	8115
14	142	54	224	94	269	440	451	milky	
15	146	55	225	95	270	450	455	nebulosity	
16	149	56	226	96	271	460	458		
17	152	57	228	97	272	470	461		
18	155	58	229	98	273	480	464		
19	158	59	230	99	274	490	468		
20	160	60	232	100	275	500	471		
21	163	61	233	110	284	510	474		
22	166	62	234	120	291	520	477		
23	168	63	236	130	300	530	480		
24	170	64	237	140	308	540	483		
25	173	65	238	150	315	550	486		
26	175	66	239	160	322	560	489		
27	177	67	240	170	328	570	492		
28	180	68	242	180	335	580	495		
29	182	69	243	190	341	590	498		
30	184	70	244	200	347	600	500		

Section

Section of our ſidereal ſyſtem.

By taking out of this table the viſual rays which anſwer to the gages, and applying lines proportional to them around a point, according to their reſpective right aſcenſions and north polar diſtances, we may delineate a ſolid by means of the ends of theſe lines, which will give us ſo many points in its ſurface; I ſhall, however, content myſelf at preſent with a ſection only. I have taken one which paſſes through the poles of our ſyſtem, and is at rectangles to the conjunction of the branches which I have called its length. The name of poles ſeemed to me not improperly applied to thoſe points which are 90 degrees diſtant from a circle paſſing along the milky way, and the north pole is here aſſumed to be ſituated in R.A. 186° and P.D. 58°. The ſection repreſented in fig. 4. is one which makes an angle of 35 degrees with our equator, croſſing it in 124½ and 304½ degrees. A celeſtial globe, adjuſted to the latitude of 55° north, and having σ Ceti near the meridian, will have the plane of this ſection pointed out by the horizon, and the gages which have been uſed in this delineation are thoſe which in table I. are marked by aſteriſks. When the viſual rays anſwering to them are taken out of the ſecond table, they muſt be projected on the plane of the horizon of the latitude which has been pointed out; and this may be done accurately enough for the preſent purpoſe by a globe adjuſted as above directed; for as gages, exactly in the plane of the ſection, were often wanting, I have uſed many at ſome ſmall diſtance above and below the ſame, for the ſake of obtaining more delineating points; and in the figure the ſtars at the borders which are larger than the reſt are thoſe pointed out by the gages. The

intermediate parts are filled up by ſmaller ſtars arranged in ſtraight lines between the gaged ones. The delineating points, though pretty numerous, are not ſo cloſe as we might wiſh; it is however to be hoped that in ſome future time this branch of aſtronomy will become more cultivated, ſo that we may have gages for every quarter of a degree of the heavens at leaſt, and theſe often repeated in the moſt favourable circumſtances. And whenever that ſhall be the caſe, the delineations may then be repeated with all the accuracy that long experience may enable us to introduce; for, this ſubject being ſo new, I look upon what is here given partly as only an example to illuſtrate the ſpirit of the method. From this figure however, which I hope is not a very inaccurate one, we may ſee that our nebula, as we obſerved before, is of the third form; that is: *A very extenſive, branching, compound Congeries of many millions of ſtars*; which moſt probably owes its origin to many remarkably large as well as pretty cloſely ſcattered ſmall ſtars, that may have drawn together the reſt. Now, to have ſome idea of the wonderful extent of this ſyſtem, I muſt obſerve that this ſection of it is drawn upon a ſcale where the diſtance of Sirius is no more than the 80th part of an inch; ſo that probably all the ſtars, which in the fineſt nights we are able to diſtinguiſh with the naked eye, may be comprehended within a ſphere, drawn round the large ſtar near the middle, repreſenting our ſituation in the nebula, of leſs than half a quarter of an inch radius.

The Origin of nebulous Strata.

If it were poſſible to diſtinguiſh between the parts of an indefinitely extended whole, the nebula we inhabit might be ſaid

ſaid to be one that has fewer marks of profound antiquity upon it than the reſt. To explain this idea perhaps more clearly, we ſhould recollect that the condenſation of cluſters of ſtars has been aſcribed to a gradual approach; and whoever reflects on the numbers of ages that muſt have paſt before ſome of the cluſters, that will be found in my intended catalogue of them, could be ſo far condenſed as we find them at preſent, will not wonder if I aſcribe a certain air of youth and vigour to many very regularly ſcattered regions of our ſidereal ſtratum. There are moreover many places in it where there is the greateſt reaſon to believe that the ſtars, if we may judge from appearances, are now drawing towards various ſecondary centers, and will in time ſeparate into different cluſters, ſo as to occaſion many ſub-diviſions. Hence we may ſurmiſe that when a nebulous ſtratum conſiſts chiefly of nebulæ of the firſt and ſecond form, it probably owes its origin to what may be called the decay of a great compound nebula of the third form; and that the ſub-diviſions, which happened to it in length of time, occaſioned all the ſmall nebulæ which ſprung from it to lie in a certain range, according as they were detached from the primary one. In like manner our ſyſtem, after numbers of ages, may very poſſibly become divided ſo as to give riſe to a ſtratum of two or three hundred nebulæ; for it would not be difficult to point out ſo many beginning or gathering cluſters in it (*f*). This view of the preſent ſubject throws a conſiderable light upon the appearance of that remarkable collection of many

(*f*) Mr. MICHELL has alſo conſidered the ſtars as gathered together into groups (Phil. Tranſ. vol. LVII. p. 249.); which idea agrees with the ſub-diviſion of our great ſyſtem here pointed out. He founds an elegant proof of this on the computation of probabilities, and mentions the Pleiades, the Præſepe Cancri, and the nebula (or cluſter of ſtars) in the hilt of Perſeus's ſword, as inſtances.

hundreds

hundreds of nebulæ which are to be ſeen in what I have called the nebulous ſtratum of Coma Berenices. It appears from the extended and branching figure of our nebula, that there is room for the decompoſed ſmall nebulæ of a large, reduced, former great one to approach nearer to us in the ſides than in other parts. Nay, poſſibly, there might originally be another very large joining branch, which in time became ſeparated by the condenſation of the ſtars; and this may be the reaſon of the little remaining breadth of our ſyſtem in that very place: for the nebulæ of the ſtratum of the Coma are brighteſt and moſt crowded juſt oppoſite our ſituation, or in the pole of our ſyſtem. As ſoon as this idea was ſuggeſted, I tried alſo the oppoſite pole, where accordingly I have met with a great number of nebulæ, though under a much more ſcattered form.

An Opening in the heavens.

Some parts of our ſyſtem indeed ſeem already to have ſuſtained greater ravages of time than others, if this way of expreſſing myſelf may be allowed; for inſtance, in the body of the Scorpion is an opening, or hole, which is probably owing to this cauſe. I found it while I was gaging in the parallel from 112 to 114 degrees of north polar diſtance. As I approached the milky way, the gages had been gradually running up from 9,7 to 17,1; when, all of a ſudden, they fell down to nothing, a very few pretty large ſtars excepted, which made them ſhew 0,5, 0,7, 1,1, 1,4, 1,8; after which they again roſe to 4,7, 13,5, 20,3, and ſoon after to 41,1. This opening is at leaſt 4 degrees broad, but its height I have not yet aſcertained. It is remarkable, that the 80 *Nebuleuſe ſans étoiles* of the *Connoiſſance des Temps*, which is one of the richeſt and moſt compreſſed

preſſed cluſters of ſmall ſtars I remember to have ſeen, is ſituated juſt on the weſtern border of it, and would almoſt authoriſe a ſuſpicion that the ſtars, of which it is compoſed, were collected from that place, and had left the vacancy. What adds not a little to this ſurmiſe is, that the ſame phænomenon is once more repeated with the fourth cluſter of ſtars of the *Connoiſſance des Temps*; which is alſo on the weſtern border of another vacancy, and has moreover a ſmall, miniature cluſter, or eaſily reſolvable nebula of about $2\frac{1}{2}$ minutes in diameter, north following it, at no very great diſtance.

Phænomena at the Poles of our Nebula.

I ought to obſerve, that there is a remarkable purity or clearneſs in the heavens when we look out of our ſtratum at the ſides; that is, towards Leo, Virgo, and Coma Berenices, on one hand, and towards Cetus on the other; whereas the ground of the heavens becomes troubled as we approach towards the length or height of it. It was a good while before I could trace the cauſe of theſe phænomena; but ſince I have been acquainted with the ſhape of our ſyſtem, it is plain that theſe troubled appearances, when we approach to the ſides, are eaſily to be explained by aſcribing them to ſome of the diſtant, ſtraggling ſtars, that yield hardly light enough to be diſtinguiſhed. And I have, indeed, often experienced this to be actually the cauſe, by examining theſe troubled ſpots for a long while together, when, at laſt, I generally perceived the ſtars which occaſioned them. But when we look towards the poles of our ſyſtem, where the viſual ray does not graze along the ſide, the

ſtraggling ſtars of courſe will be very few in number; and therefore the ground of the heavens will aſſume that purity which I have always obſerved to take place in thoſe regions.

Enumeration of very compound Nebulæ or Milky-Ways.

As we are uſed to call the appearance of the heavens, where it is ſurrounded with a bright zone, the Milky-Way, it may not be amiſs to point out ſome other very remarkable Nebulæ which cannot well be leſs, but are probably much larger than our own ſyſtem; and, being alſo extended, the inhabitants of the planets that attend the ſtars which compoſe them muſt likewiſe perceive the ſame phænomena. For which reaſon they may alſo be called milky-ways by way of diſtinction.

My opinion of their ſize is grounded on the following obſervations. There are many round nebulæ, of the firſt form, of about five or ſix minutes in diameter, the ſtars of which I can ſee very diſtinctly; and on comparing them with the viſual ray calculated from ſome of my long gages, I ſuppoſe, by the appearance of the ſmall ſtars in thoſe gages, that the centers of theſe round nebulæ may be 600 times the diſtance of Sirius from us.

In eſtimating the diſtance of ſuch cluſters I conſulted rather the comparatively apparent ſize of the ſtars than their mutual diſtance; for the condenſation in theſe cluſters being probably much greater than in our own ſyſtem, if we were to overlook this circumſtance and calculate by their apparent compreſſion, where, in about ſix minutes diameter, there are perhaps ten or more ſtars in the line of meaſures, we ſhould find, that on the ſuppoſition of an equal ſcattering of the ſtars throughout all nebulæ, the diſtance of the center of ſuch a cluſter from us could not be leſs than 6000 times the diſtance of

of Sirius. And, perhaps, in putting it, by the apparent ſize of the ſtars, at 600 only, I may have conſiderably under-rated it; but my argument, if that ſhould be the caſe, will be ſo much the ſtronger. Now to proceed,

Some of theſe round nebulæ have others near them, perfectly ſimilar in form, colour, and the diſtribution of ſtars, but of only half the diameter: and the ſtars in them ſeem to be doubly crowded, and only at about half the diſtance from each other: they are indeed ſo ſmall as not to be viſible without the utmoſt attention. I ſuppoſe theſe miniature nebulæ to be at double the diſtance of the firſt. An inſtance, equally remarkable and inſtructive, is a caſe where, in the neighbourhood of two ſuch nebulæ as have been mentioned, I met with a third, ſimilar, reſolvable, but much ſmaller and fainter nebula. The ſtars of it are no longer to be perceived; but a reſemblance of colour with the former two, and its diminiſhed ſize and light, may well permit us to place it at full twice the diſtance of the ſecond, or about four or five times that of the firſt. And yet the nebuloſity is not of the milky kind; nor is it ſo much as difficultly reſolvable, or colourleſs. Now, in a few of the extended nebulæ, the light changes gradually ſo as from the reſolvable to approach to the milky kind; which appears to me an indication that the milky light of nebulæ is owing to their much greater diſtance. A nebula, therefore, whoſe light is perfectly milky, cannot well be ſuppoſed to be at leſs than ſix or eight thouſand times the diſtance of Sirius; and though the numbers here aſſumed are not to be taken otherwiſe than as very coarſe eſtimates, yet an extended nebula, which in an oblique ſituation, where it is poſſibly fore-ſhortened by one-half, two-thirds, or three-fourths of its length, ſubtends a degree or more in

diameter, cannot be otherwiſe than of a wonderful magnitude, and may well outvie our milky-way in grandeur.

The firſt I ſhall mention is a milky Ray of more than a degree in length. It takes *k* (FL. 52.) Cygni into its extent, to the north of which it is crookedly bent ſo as to be convex towards the following ſide; and the light of it is pretty intenſe. To the ſouth of *k* it is more diffuſed, leſs bright, and loſes itſelf with ſome extenſion in two branches, I believe; but for want of light I could not determine this circumſtance. The northern half is near two minutes broad, but the ſouthern is not ſufficiently defined to aſcertain its breadth.

The next is an extremely faint milky Ray, above $\frac{3}{4}$ degree long, and 8 or 10′ broad; extended from north preceding to ſouth following. It makes an angle of about 30 or 40 degrees with the meridian, and contains three or four places that are brighter than the reſt. The ſtars of the Galaxy are ſcattered over it in the ſame manner as over the reſt of the heavens. It follows ε Cygni 11,5 minutes in time, and is 2° 19′ more ſouth.

The third is a branching Nebuloſity of about a degree and a half in right aſcenſion, and about 48′ extent in polar diſtance. The following part of it is divided into ſeveral ſtreams and windings, which, after ſeparating, meet each other again towards the ſouth. It precedes ζ Cygni 16′ in time, and is 1° 16′ more north. I ſuppoſe this to be joined to the preceding one; but having obſerved them in different ſweeps, there was no opportunity of tracing their connection.

The fourth is a faint, extended milky Ray of about 17′ in length, and 12′ in breadth. It is brighteſt and broadeſt in the middle, and the ends loſe themſelves. It has a ſmall, round, very faint nebula juſt north of it; and alſo, in another place, a ſpot, brighter than the reſt, almoſt detached enough to form

a different

a different nebula, but probably belonging to the great one. The Ray precedes α Trianguli 18′,8 in time, and is 55′ more north. Another obſervation of the ſame, in a finer evening, mentions its extending much farther towards the ſouth, and that the breadth of it probably is not leſs than half a degree; but being ſhaded away by imperceptible gradations, it is difficult exactly to aſſign its limits.

The fifth is a Streak of light about 27′ long, and in the brighteſt part 3 or 4′ broad. The extent is nearly in the meridian, or a little from ſouth preceding to north following. It follows β Ceti 5′,9 in time, and is 2° 43′ more ſouth. The ſituation is ſo low, that it would probably appear of a much greater extent in a higher altitude.

The ſixth is an extenſive milky Nebuloſity divided into two parts; the moſt north being the ſtrongeſt. Its extent exceeds 15′; the ſouthern part is followed by a parcel of ſtars which I ſuppoſe to be the 8th of the *Connoiſſance des Temps*.

The ſeventh is a wonderful, extenſive Nebuloſity of the milky kind. There are ſeveral ſtars viſible in it, but they can have no connection with that nebuloſity, and are, doubtleſs, belonging to our own ſyſtem ſcattered before it. It is the 17th of the *Connoiſſance des Temps*.

In the liſt of theſe muſt alſo be reckoned the beautiful Nebula of Orion. Its extent is much above one degree; the eaſtern branch paſſes between two very ſmall ſtars, and runs on till it meets a very bright one. Cloſe to the four ſmall ſtars, which can have no connection with the nebula, is a total blackneſs; and within the open part, towards the north-eaſt, is a diſtinct, ſmall, faint nebula, of an extended ſhape, at a diſtance from the border of the great one, to which it runs in a parallel

direction, reſembling the ſhoals that are ſeen near the coaſts of ſome iſlands.

The ninth is that in the girdle of Andromeda, which is undoubtedly the neareſt of all the great nebulæ; its extent is above a degree and a half in length, and, in even one of the narroweſt places, not leſs than 16′ in breadth. The brighteſt part of it approaches to the reſolvable nebuloſity, and begins to ſhew a faint red colour; which, from many obſervations on the colour and magnitude of nebulæ, I believe to be an indication that its diſtance in this coloured part does not exceed 2000 times the diſtance of Sirius. There is a very conſiderable, broad, pretty faint, ſmall nebula near it; my Siſter diſcovered it Auguſt 27, 1783, with a Newtonian 2-feet ſweeper. It ſhews the ſame faint colour with the great one, and is, no doubt, in the neighbourhood of it. It is not the 32d of the *Connoiſſance des Temps*; which is a pretty large round nebula, much condenſed in the middle, and ſouth following the great one; but this is about two-thirds of a degree north preceding it, in a line parallel to β and ν Andromedæ.

To theſe may be added the nebula in Vulpecula: for, though its appearance is not large, it is probably a double ſtratum of ſtars of a very great extent, one end whereof is turned towards us. That it is thus ſituated may be ſurmiſed from its containing, in different parts, nearly all the three nebuloſities; *viz.* the reſolvable, the coloured but irreſolvable, and a tincture of the milky kind. Now, what great length muſt be required to produce theſe effects may eaſily be conceived when, in all probability, our whole ſyſtem, of about 800 ſtars in diameter, if it were ſeen at ſuch a diſtance that one end of it might aſſume the reſolvable nebuloſity, would not, at the other end, preſent

us

us with the irreſolvable, much leſs with the colourleſs and milky ſort of nebuloſities.

A Perforated Nebula, or Ring of Stars.

Among the curioſities of the heavens ſhould be placed a nebula, that has a regular, concentric, dark ſpot in the middle, and is probably a Ring of ſtars. It is of an oval ſhape, the ſhorter axis being to the longer as about 83 to 100; ſo that, if the ſtars form a circle, its inclination to a line drawn from the ſun to the center of this nebula muſt be about 56 degrees. The light is of the reſolvable kind, and in the northern ſide three very faint ſtars may be ſeen, as alſo one or two in the ſouthern part. The vertices of the longer axis ſeem leſs bright and not ſo well defined as the reſt. There are ſeveral ſmall ſtars very near, but none that ſeem to belong to it. It is the 57th of the *Connoiſſance des Temps*. Fig. 5. is a repreſentation of it.

Planetary Nebulæ.

I ſhall conclude this paper with an account of a few heavenly bodies, that from their ſingular appearance leave me almoſt in doubt where to claſs them.

The firſt precedes ν Aquarii 5′,4 in time, and is 1′ more north. Its place, with regard to a ſmall ſtar Sept. 7, 1782, was, Diſtance 8′ 13″ 51‴; but on account of the low ſituation, and other unfavourable circumſtances, the meaſure cannot be very exact. Auguſt 25, 1783, Diſtance 7′ 5″ 11‴, very exact, and to my ſatisfaction; the light being thrown in by an opaque-microſcopic-illumination (*g*). Sept. 20, 1783, Poſition 41° 24′ ſouth

(*g*) It may be of uſe to explain this kind of illumination for which the Newtonian reflector is admirably conſtructed. On the ſide oppoſite the eye-piece an opening is to be made in the tube, through which the light may be thrown in, ſo as to fall on ſome reflecting body, or concave perforated mirror, within the eye-piece,

ſouth preceding the ſame ſtar; very exact, and by the ſame kind of illumination. Oct. 17, 1783, Diſtance 6′ 55″ 7‴; a ſecond meaſure 6′ 56″ 11‴, as exact as poſſible. Oct. 23, 1783, Poſition 42° 57′; a ſecond meaſure 42° 45′; ſingle lens; power 71; opaque-microſcopic-illumination. Nov. 14, 1783, Diſtance 7′ 4″ 35‴. Nov. 12, 1784, Diſtance 7′ 22″ 35‴; Poſition 38° 39′. Its diameter is about 10 or 15″. I have examined it with the powers of 71, 227, 278, 460, and 932; and it follows the laws of magnifying, ſo that its body is no illuſion of light. It is a little oval, and in the 7-feet reflector pretty well defined, but not ſharp on the edges. In the 20-feet, of 18,7 inch aperture, it is much better defined, and has much of a planetary appearance, being all over of an uniform brightneſs, in which it differs from nebulæ: its light ſeems however to be of the ſtarry nature, which ſuffers not nearly ſo much as the planetary diſks are known to do, when much magnified.

The ſecond of theſe bodies precedes the 13th of FLAMSTEED's Andromeda about 1′6 in time, and is 22′ more ſouth. It has a round, bright, pretty well defined planetary diſk of about 12″ diameter, and is a little elliptical. When it is viewed with a 7-feet reflector, or other inferior inſtruments, it is not nearly ſo well defined as with the 20-feet. Its ſituation with regard to a pretty conſiderable ſtar is, Diſtance (with a compound glaſs of a low power) 7′ 51″ 34‴. Poſition 12° 0′ f. preceding. Diameter taken with 278, 14″ 42‴.

The third follows B (FL. 44.) Ophiuchi 4′,1 in time, and is 23′ more north. It is round, tolerably well defined, and pretty bright; its diameter is about 30″.

piece, that may throw it back upon the wires. By this means none of the direct rays can reach the eye, and thoſe few which are reflected again from the wires do not interfere ſenſibly with the fainteſt objects, which may thus be ſeen undiſturbed.

The

The fourth follows η Sagittæ 17′,1 in time, and is 2′ more north. It is perfectly round, pretty bright, and pretty well defined; about $\frac{3}{4}$ min. in diameter.

The fifth follows the 21ſt Vulpeculæ 2′,1 in time, and is 1° 46′ more north. It is exactly round, of an equal light throughout, but pretty faint, and about 1′ in diameter.

The ſixth precedes *h* (Fl. 39.) Cygni 8′,1 in time, and is 1° 26′ more ſouth. It is perfectly round, and of an equal light, but pretty faint; its diameter is near 1′, and the edges are pretty well defined.

The planetary appearance of the two firſt is ſo remarkable, that we can hardly ſuppoſe them to be nebulæ; their light is ſo uniform, as well as vivid, the diameters ſo ſmall and well defined, as to make it almoſt improbable they ſhould belong to that ſpecies of bodies. On the other hand, the effect of different powers ſeems to be much againſt their light's being of a planetary nature, ſince it preſerves its brightneſs nearly in the ſame manner as the ſtars do in ſimilar trials. If we would ſuppoſe them to be ſingle ſtars with large diameters we ſhall find it difficult to account for their not being brighter; unleſs we ſhould admit that the intrinſic light of ſome ſtars may be very much inferior to that of the generality, which however can hardly be imagined to extend to ſuch a degree. We might ſuſpect them to be comets about their aphelion, if the brightneſs as well as magnitude of the diameters did not oppoſe this idea; ſo that after all, we can hardly find any hypotheſis ſo probable as that of their being Nebulæ; but then they muſt conſiſt of ſtars that are compreſſed and accumulated in the higheſt degree. If it were not perhaps too hazardous to purſue a former ſurmiſe of a renewal in what I figuratively called the Laboratories of the univerſe, the ſtars forming theſe extraordinary nebulæ, by ſome decay or waſte of nature, being no longer

fit for their former purpoſes, and having their projectile forces, if any ſuch they had, retarded in each others atmoſphere, may ruſh at laſt together, and either in ſucceſſion, or by one general tremendous ſhock, unite into a new body. Perhaps the extraordinary and ſudden blaze of a new ſtar in Caſſiopea's chair, in 1572, might poſſibly be of ſuch a nature. But leſt I ſhould be led too far from the path of obſervation, to which I am reſolved to limit myſelf, I ſhall only point out a conſiderable uſe that may be made of theſe curious bodies. If a little attention to them ſhould prove that, having no annual parallax, they belong moſt probably to the claſs of nebulæ, they may then be expected to keep their ſituation better than any one of the ſtars belonging to our ſyſtem, on account of their being probably at a very great diſtance. Now to have a fixed point ſomewhere in the heavens, to which the motions of the reſt may be referred, is certainly of conſiderable conſequence in Aſtronomy; and both theſe bodies are bright and ſmall enough to anſwer that end (*b*).

Datchet near Windſor,
January 1, 1785.

W. HERSCHEL.

(*b*) Having found two more of theſe curious objects, I add the place of them here, in hopes that thoſe who have fixed inſtruments may be induced to take an early opportunity of obſerviug them carefully.

Feb. 1, 1785. A very bright, planetary nebula, about half a minute in diameter, but the edges are not very well defined. It is perfectly round, or perhaps a very little elliptical, and all over of an uniform brightneſs: with higher powers it becomes proportionally magnified. It follows γ Eridani 16′ 16″ in time, and is 49′ more north than that ſtar.

Feb. 7, 1785. A beautiful, very brilliant globe of light; a little hazy on the edges, but the hazineſs goes off very ſuddenly, ſo as not to exceed the 20th part of the diameter, which I ſuppoſe to be from 30 to 40″. It is round, or perhaps a very little elliptical, and all over of an uniform brightneſs: I ſuppoſe the intenſity of its light to be equal to that of a ſtar of the ninth magnitude. It precedes the third *b* (FL. 6.) Crateris 28′ 36″ in time, and is 1° 25′ more north than that ſtar.

Fig. 1.

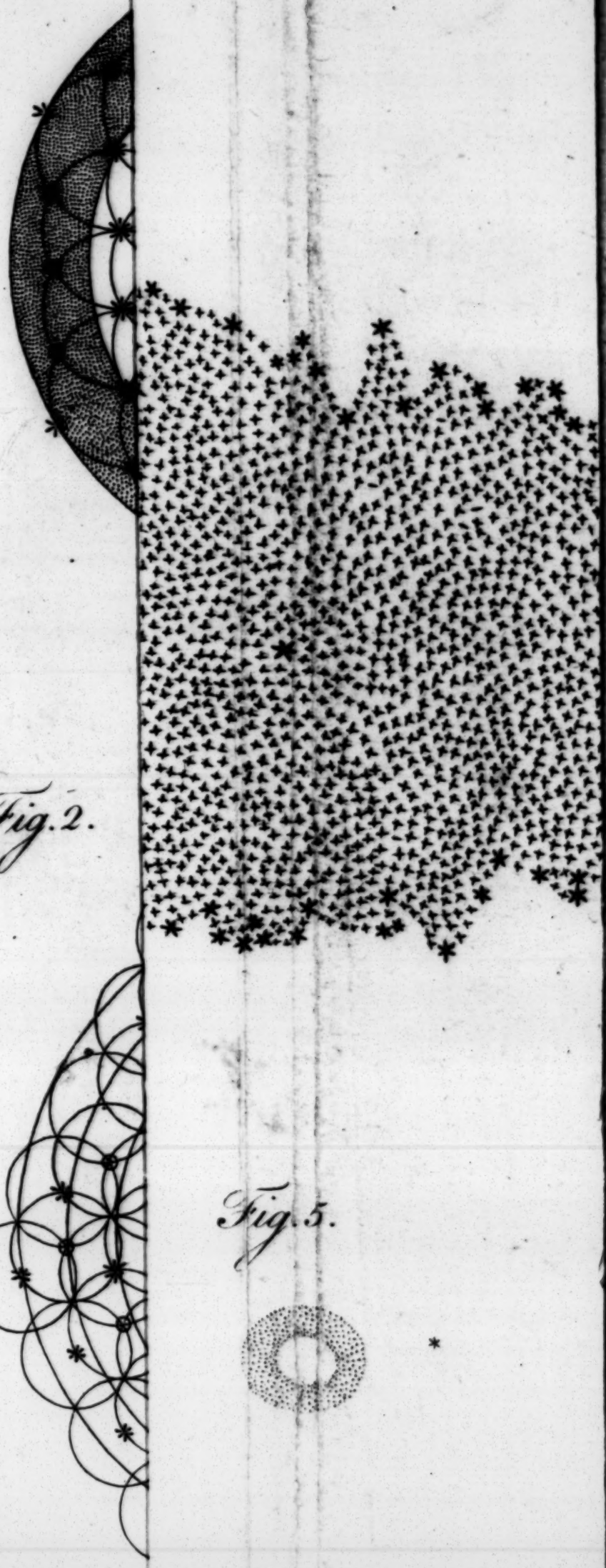

Fig. 2.

Fig. 3.

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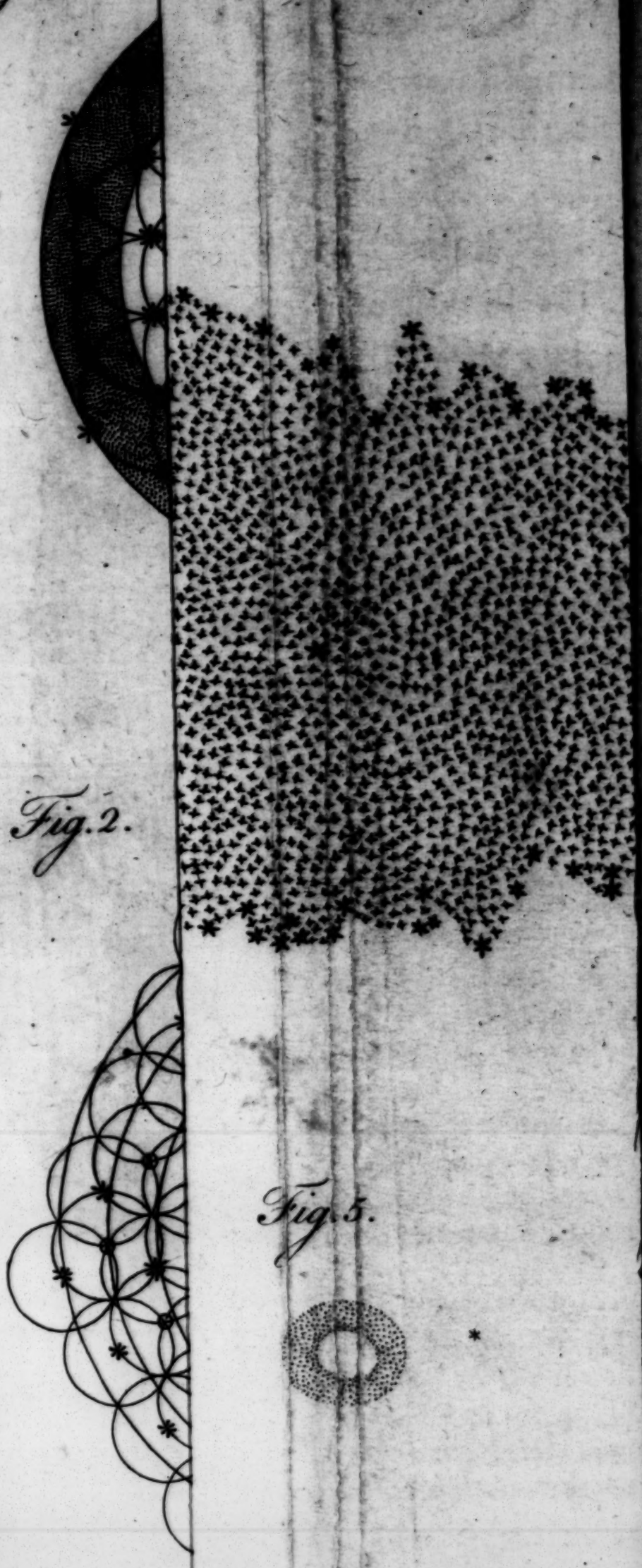

Fig. 2.

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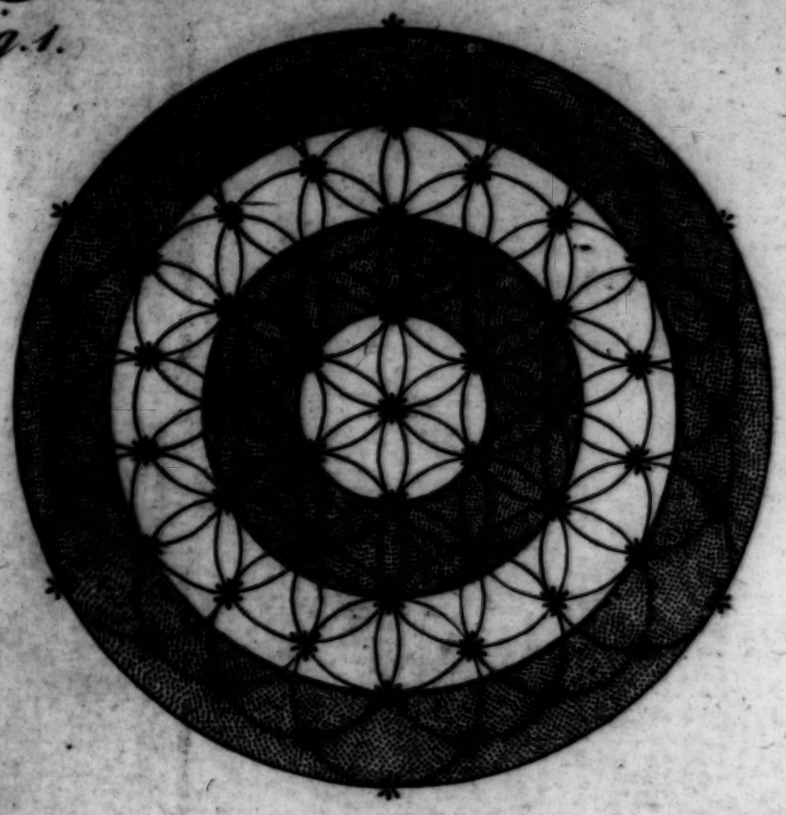

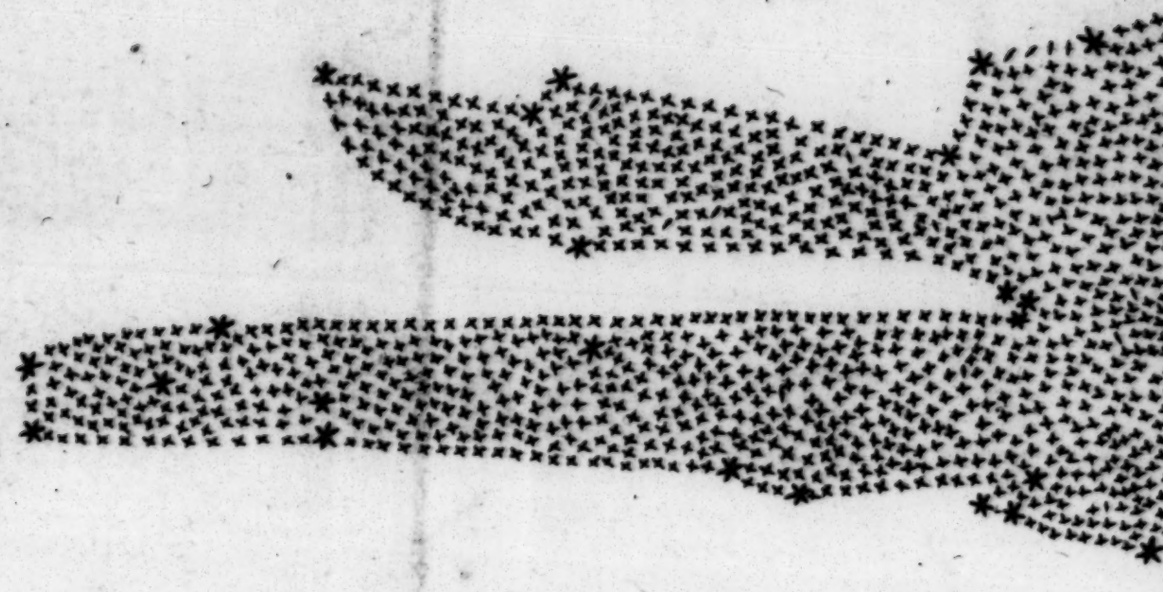

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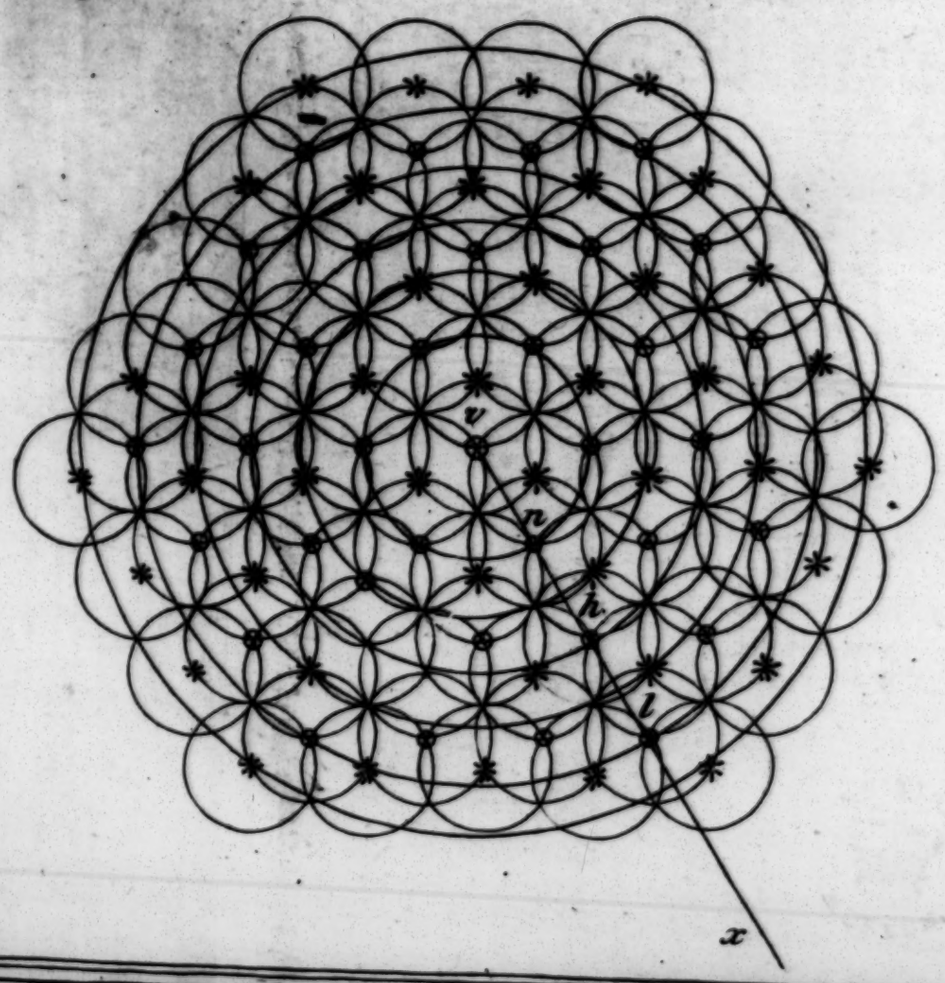

Fig. 3.

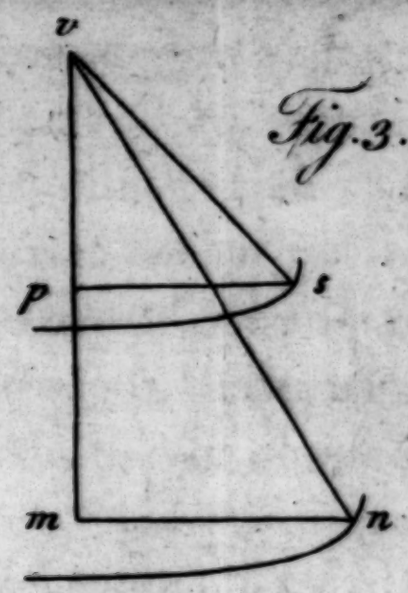

Fig. 4.

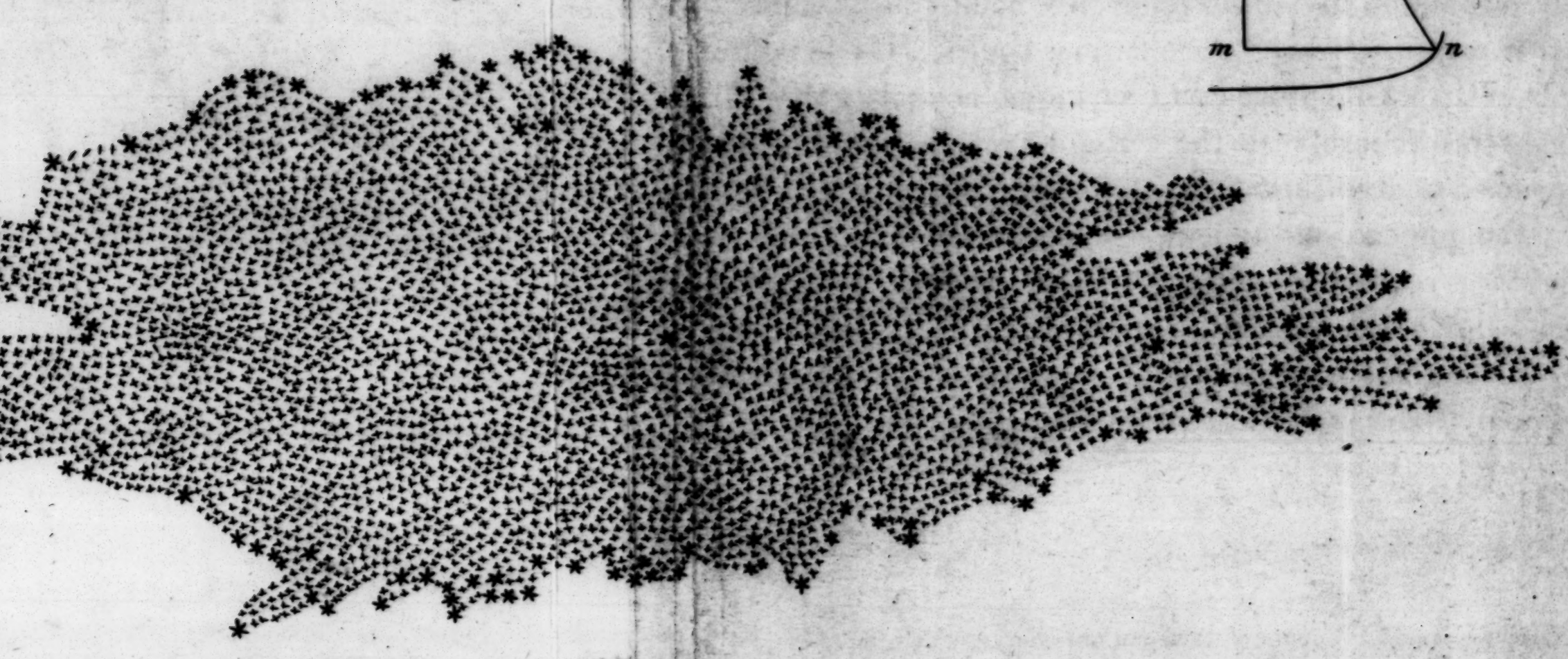

Fig. 5.

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XIII. *Remarks on ſpecific Gravities taken at different Degrees of Heat, and an eaſy Method of reducing them to a common Standard.* *By* Richard Kirwan, *Eſq. F. R. S.*

Read February 17, 1785.

THAT a comparative view of the weights of equal volumes of water and all other ſubſtances is highly uſeful on many occaſions, is too well known to require any proof; but that a principal uſe reſulting from this compariſon, when properly made, is unattainable by a peruſal of the common tables, I ſhall here endeavour to ſhew, and at the ſame time point out a remedy for this defect.

One capital advantage derivable from a table of ſpecific gravities, is the knowledge of the abſolute weight of any ſolid meaſure of the ſubſtances therein contained, or that of the ſolid meaſure of a given weight of thoſe ſubſtances, a cubic foot of water being ſuppoſed to weigh 1000 ounces avoirdupois, and conſequently a cubic inch of water weighing 253,182 grains. But the authors who have diſcovered this equation of weight and meaſure, and all thoſe who have ſince treated this ſubject, have neglected to inform us of the temperature at which this agreement takes place; yet that it cannot take place in all temperatures is evident from the experiments of Dr. HALLEY and others, who have found, that from a few degrees above the freezing to the boiling point, water is dilated about $\frac{1}{26}$ of its bulk; and, conſequently, if 1000 ounces at the freezing point be equal to one cubic foot, they muſt be equal at the boiling

point to one cubic foot and 66,46 cubic inches. And if the dilatations are proportional to the degrees of heat throughout the ſcale, there muſt be an augmentation of 3,136 cubic inches *per* cubic foot, produced by every 10 degrees of heat. Both theſe points remain, therefore, to be determined; firſt, at what temperature a cubic foot of water weighs exactly 1000 ounces avoirdupois; and, 2dly, whether the dilatations produced by ſucceſſive degrees of heat are proportional to the degrees that produce them. This laſt point has indeed been handled by others, but with different views; and their determinations are not eaſily applicable to the preſent queſtion.

To examine this matter experimentally, I ordered a hollow tinned iron cone to be made, of four inches diameter in the baſe, one-tenth of an inch diameter in the ſummit inſide, and 10 inches perpendicular height, whoſe ſolid contents ſhould be 42,961 cubic inches, but by a ſlight diminution of the diameter, and a protuberance ariſing from the ſoldering, I found it to contain, in the temperature of 62°, but 42,731 cubic inches, according to the eſtimation of 1000 ounces to the cubic foot; and having filled it by immerſion in boiling water, and taken it up at different degrees of heat, and weighed it when cold, I found its contents as expreſſed in the following table; the firſt column of which ſhews the degrees of heat at which it was taken up; the ſecond, the weight of the water contained in it; the third, the diminution of weight occaſioned by thoſe degrees of heat; the fourth, the ſum of the diminutions of weight in the cubic foot, by the preceding degrees of heat; the fifth ſhews the weight of a cubic inch of water in each of thoſe degrees of heat; and the ſixth, the augmentation of bulk in the cubic foot by every 20° of heat. The horizontal lines, marked thus *, I have added from the experiments of Mr.

Bladh, in the Memoirs of the Academy of Stockholm for the year 1776, whoſe determinations, as far as they reached, agreed very nearly with mine. The water I uſed was common water well boiled and filtered. The experiments were for the moſt part three times repeated, and the difference in each trial amounted to a very few grains.

I.	II.	III.	IV.	V.	VI.	
Degrees	Contents of the eone in grains.	Dimin. in grains.	Sum of dim. in a cubic foot.	Weight of a cubic inch.	Increaſe in cubic inches.	
			Grs.			
212	10418,75	29,5	16589	243,8	4,892	
202	10448,25	77,5	15354	244,51	12,818	
182	10525,75	71,75	12133	246,33	11,533	
162	10596,00	62,60	9171	247,97	10,209	
142	10658,60	56,15	6602	249,43	9,103	
122	10714,75	49,00	4310	250,75	7,920	
102	10763,75	35,5	2226	251,89	5,7	
82	10799,25	19,5	788	252,72	3,120	
*75	— — —	— —	— — —	252,8	— — —	
*70	— — —	— —	— — —	252,97	— — —	
*66	— — —	— —	— — —	253,06	— — —	
62	10818,75	0	0	253,182	0	Total increaſe of bulk from 62° to 212° = 65,327 cubic inches.
*56	— — —	— —	— — —	253,3	— — —	
		Increaſe	Increaſe			
*50	— — —	— —	— — —	253,46	— — —	
					Decreaſe	Total from 36° to 212 = 67,327 cubic inches.
42	10830,75	12	485,3	253,463	1,936	
*36,5	— — —	— —	— — —	253,5	0,064	

Hence we ſee, that a cubic foot of water weighs 485,3 grains more at 42° than at 62°, and conſequently is equal to 1001,109 avoirdupois ounces, and in the temperature of 82° it weighs leſs than at 62° by 788,5 grains, and therefore is equal to 998,198 ounces. At the boiling point it wants 16589 grains, or 37,915 ounces of the weight it poſſeſſes at 62°, and conſequently weighs but 962,085 ounces, &c.

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122	10714,75	49,00	4310	250,75	7,920	
102	10763,75	35,5	2226	251,89	5,7	
82	10799,25	19,5	788	252,72	3,120	
*75	– – –	– –	– – –	252,8	– – –	
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In this calculation I take no account of the difference ariſing from the expanſion of the veſſel, it being only 0,067 of an inch at the boiling point; for, according to BOUGUER, iron is dilated 0,00055 of its bulk from the freezing to the boiling point; conſequently 42,961 cubic inches gain only 0,067 of an inch, augmenting the diameter and perpendicular height of this fruſtum of a cone at the boiling point in that proportion.

Hence alſo we ſee, that the expanſions of water are not proportional to the degrees of heat; for by 20 degrees of heat from 62° to 82° a cubic foot of water is dilated only 3,12 inches, but by the next 20 degrees of heat, that is, from 82° to 102°, it is expanded 5,7 inches, &c.

Mr. BLADH found the volume of water at 32° to be equal to that at 53°,6; but that this irregular expanſion ceaſed at 36°6, and, according to Mr. DE LUC (who firſt diſcovered it) at 43°.

As the expanſion of liquids by equal degrees of heat is much greater than that of ſolids, it happens, that the ſpecific gravities of the ſame ſolid taken at different temperatures will be different; and, what appears more extraordinary, the ſame ſolid will appear ſpecifically heavier in higher than in lower temperatures; for the ſame volume of water being lighter in higher than in lower temperatures, the ſolid will loſe leſs of its weight in it in the former than in the latter caſe: this miſtake we may remedy by inſpecting the fifth column of the foregoing table and the following analogy: as the weight of a cubic inch of water at the temperature of 62° is to the weight of a cubic inch of water at *n* degrees of temperature, ſo is the ſpecific gravity found at *n* degrees of temperature to that which will be found at 62°.

Thus, if 1000 grains of iron be weighed in water of the temperature of 62°, and it loſes therein 13,333 grains, if the

ſame

ſame piece of iron be weighed in water of the temperature of 75°, it will loſe but 13,313 grains; for the loſſes of weight will be as the weights of equal volumes of water at thoſe temperatures, which, as we have ſeen, are as 253,18 to 252,8; therefore, its ſpecific gravity in water of the temperature of 62° will be 7,49; and in water of the temperature of 75°. 7,511; but we may correct this by the above analogy, for ∴ 253,8 . 252,18 :: 7,511 . 7,49.

By this means we obtain the advantage of diſcovering the true weight of a cubic foot of any ſubſtance whoſe ſpecific gravity is known, which it is now plain cannot be known when bodies are hydroſtatically weighed at any temperature a few degrees above or below 62°, without ſuch reduction, or ſubtracting the quantities in the fourth column.

This method is equally applicable, and with equal neceſſity, to other means of finding ſpecific gravities, as areometers, the compariſon of the weights of equal meaſures of liquids, the different loſſes of weight of the ſame ſolid, when weighed in different liquids, &c. In all which caſes the weight of water at 62°, or the loſs of weight of a ſolid in water at 62°, ſhould be found by the above analogy.

Dr. HALES and ſome others have eſtimated the weight of a cubic inch of water at 254 grains, which is an evident miſtake, as it is true in no degree of temperature, and produces an error of more than three ounces in the cubic foot.

XIV. *Electrical Experiments made in order to aſcertain the non-conducting Power of a perfect Vacuum*, &c. *By Mr.* William Morgan; *communicated by the Rev.* Richard Price, *LL.D. F.R.S.*

Read February 24, 1785.

THE non-conducting power of a perfect vacuum is a fact in electricity which has been much controverted among philoſophers. The experiments made by Mr. WALSH, F.R.S. in the double barometer tube clearly demonſtrated the impermeability of the electric *light* through a vacuum; nor was it, I think, precipitate to conclude from them the impermeability of the electric *fluid* itſelf. But this concluſion has not been univerſally admitted, and the following experiments were made with the view of determining its truth or fallacy. When I firſt attended to the ſubject, I was not aware that any other attempts had been made beſides thoſe of Mr. WALSH; and though I have ſince found myſelf to have been in part anticipated in one of my experiments, it may not perhaps be improper to give ſome account of them, not only as they are an additional teſtimony in ſupport of this fact, but as they led to the obſervation of ſome phænomena which appear to be new and intereſting.

A mercurial gage B (ſee tab. IX. fig. 1.) about 15 inches long, carefully and accurately boiled till every particle of air was expelled from the inſide, was coated with tin-foil five inches down from its ſealed end (A), and being inverted into mercury

mercury through a perforation (D) in the braſs cap (E) which covered the mouth of the ciſtern (H), the whole was cemented together, and the air was exhauſted from the inſide of the ciſtern through a valve (C) in the braſs cap (E) juſt mentioned, which producing a perfect vacuum in the gage (B) afforded an inſtrument peculiarly well adapted for experiments of this kind. Things being thus adjuſted (a ſmall wire (F) having been previouſly fixed on the inſide of the ciſtern to form a communication between the braſs cap (E) and the mercury (G) into which the gage was inverted) the coated end (A) was applied to the conductor of an electrical machine, and notwithſtanding every effort, neither the ſmalleſt ray of light, nor the ſlighteſt charge, could ever be procured in this exhauſted gage. I need not obſerve, that if the vacuum on its inſide had been a conductor of electricity, the latter at leaſt muſt have taken place, for it is well known (and I have myſelf often made the experiment) that if a glaſs tube be exhauſted by an air-pump, and coated on the outſide, both light and a charge may very readily be procured. If the mercury in the gage be imperfectly boiled, the experiment will not ſucceed; but the colour of the electric light, which, in air rarefied by an exhauſter, is always violet or purple, appears in this caſe of a beautiful green, and, what is very curious, the degree of the air's rarefaction may be nearly determined by this means; for I have known inſtances, during the courſe of theſe experiments, where a ſmall particle of air having found its way into the tube (B), the electric light became viſible, and as uſual of a green colour; but the charge being often repeated, the gage has at length cracked at its ſealed end, and in conſequence the external air, by being admitted into the inſide, has gradually produced a change in the electric light from green to blue, from blue to indigo, and

ſo on to violet and purple, till the medium has at laſt become ſo denſe as no longer to be a conductor of electricity. I think there can be little doubt from the above experiments of the non-conducting power of a perfect vacuum; and this fact is ſtill more ſtrongly confirmed by the phænomena which appear upon the admiſſion of a very minute particle of air into the inſide of the gage. In this caſe the whole becomes immediately luminous upon the ſlighteſt application of electricity, and a charge takes place, which continues to grow more and more powerful in proportion as freſh air is admitted, till the denſity of the conducting medium arrives at its maximum, which it always does when the colour of the electric light is indigo or violet. Under theſe circumſtances the charge may be ſo far increaſed as frequently to break the glaſs. In ſome tubes, which have not been completely boiled, I have obſerved, that they will not conduct the electric fluid when the mercury is fallen very low in them, yet upon letting in air into the ciſtern (H), ſo that the mercury ſhall riſe in the gage (B), the electric fluid, which was before latent in the inſide, ſhall now become viſible, and as the mercury continues to riſe, and of conſequence the medium is rendered leſs rare, the light ſhall grow more and more viſible, and the gage ſhall at laſt be charged, notwithſtanding it has not been near an electrical machine for two or three days. This ſeems to prove, that there is a limit even in the rarefaction of air, which ſets bounds to its conducting power; or, in other words, that the particles of air may be ſo far ſeparated from each other as no longer to be able to tranſmit the electric fluid; that if they are brought within a certain diſtance of each other, their conducting power begins, and continually increaſes till their approach alſo arrives at its limit, when the particles again become ſo near as to reſiſt the paſſage

of the fluid entirely, without employing violence, which is the caſe in common and condenſed air, but more particularly in the latter. Theſe experiments, however, belong to another ſubject, and may poſſibly be communicated at ſome future time.

It is ſurpriſing to obſerve, how readily an exhauſted tube is charged with electricity. By placing it at ten or twelve inches from the conductor the light may be ſeen pervading its inſide, and as ſtrong a charge may ſometimes be procured as if it were in contact with the conductor: nor does it ſignify how narrow the bore of the glaſs may be; for even a thermometer tube, having the minuteſt perforation poſſible, will charge with the utmoſt facility; and in this experiment the phænomena are peculiarly beautiful.

Let one end of a thermometer tube be ſealed hermetically. Let the other end be cemented into a braſs cap with a valve, or into a braſs cock, ſo that it may be fitted to the plate of an air-pump. When it is exhauſted, let the ſealed end be applied to the conductor of an electrical machine, while the other end is either held in the hand or connected to the floor. Upon the ſlighteſt excitation the electric fluid will accumulate at the ſealed end, and be diſcharged through the inſide in the form of a ſpark, and this accumulation and diſcharge may be inceſſantly repeated till the tube is broken. By this means I have had a ſpark 42 inches long, and, had I been provided with a proper tube, I do not doubt but that I might have had a ſpark of four times that length. If, inſtead of the ſealed end, a bulb be blown at that extremity of the tube, the electric light will fill the whole of that bulb, and then paſs through the tube in the form of a brilliant ſpark, as in the foregoing experiment; but in this caſe I have ſeldom been able to repeat the trials above three or four

times before the charge has made a ſmall perforation in the bulb. If again a thermometer filled with mercury be inverted into a ciſtern, and the air exhauſted in the manner I have deſcribed for making the experiment with the gage, a Torricellian vacuum will be produced; and now the electric light in the bulb, as well as the ſpark in the tube, will be of a vivid green; but the bulb will not bear a frequent repetition of charges before it is perforated in like manner as when it has been exhauſted by an air-pump. It can hardly be neceſſary to obſerve, that in theſe caſes the electric fluid aſſumes the appearance of a ſpark *, from the narrowneſs of the paſſage through which it forces its way. If a tube, 40 inches long, be fixed into a globe 8 or 9 inches in diameter, and the whole be exhauſted, the electric fluid, after paſſing in the form of a brilliant ſpark throughout the length of the tube, will, when it gets into the inſide of the globe, expand itſelf in all directions, entirely filling it with a violet and purple light, and exhibiting a ſtriking inſtance of the vaſt elaſticity of the electric fluid.

I cannot conclude this paper without acknowledging my obligations to the ingenious Mr. BROOK, of Norwich, who, by communicating to me his method of boiling mercury, has been the chief cauſe of my ſucceſs in theſe experiments †. I have lately learned

* By cementing the ſtring of a guittar into one end of a thermometer tube, a ſpark may be obtained as well as if the tube had been ſealed hermetically.

† Mr. BROOK's method of making mercurial gages is nearly as follows. Let a glaſs tube L (ſee fig. 2.), ſealed hermetically at one end, be bent into a right-angle within two or three inches of the other end. At the diſtance of about an inch or leſs from the angle let a bulb (K), of about $\frac{3}{4}$ of an inch in diameter, be blown in the curved end, and let the remainder of this part of the tube be drawn out (I) ſo

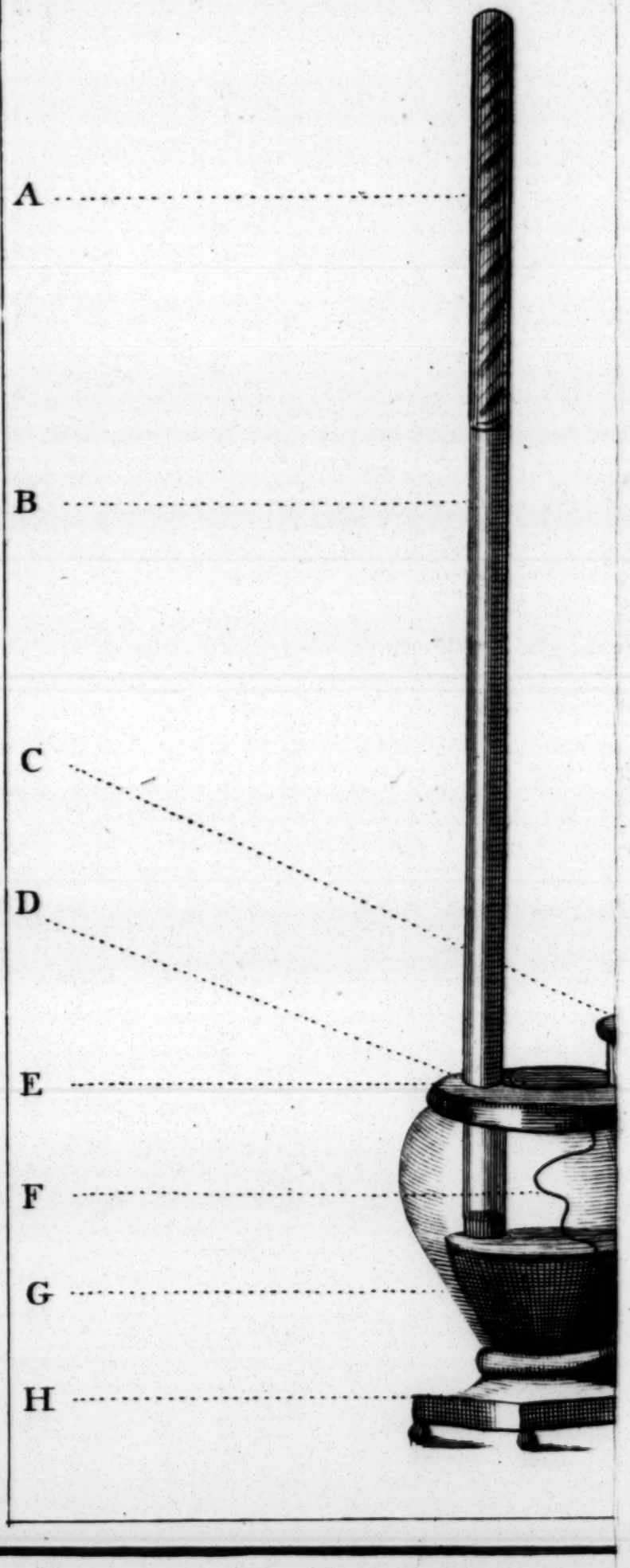
learned from him, that he has alſo aſcertained the non-conducting power of a perfect vacuum; but what ſteps he took for that purpoſe I know not. Of his accuracy, however, I am ſo well convinced, that had I never made an experiment myſelf, I ſhould, upon his teſtimony alone, have been equally aſſured of the fact. To moſt of the preceding experiments Dr. PRICE, Mr. LANE, and ſome others of my friends, have been eye-witneſſes, and I believe that they were as thoroughly ſatisfied as myſelf with the reſults of them. I muſt beg leave to obſerve to thoſe who wiſh to repeat them, that the firſt experiment requires ſome nicety, and no inconſiderable degree of labour and patience. I have boiled many gages for ſeveral hours together without ſucceſs, ſo as to be ſufficiently long to take hold of, when the mercury is boiling. The bulb (K) is deſigned as a receptacle for the mercury, to prevent its boiling over, and the bent figure of the tube is adapted for its inverſion into the ciſtern; for by breaking off the tube at (M) within $\frac{1}{8}$ or $\frac{1}{4}$ of an inch of the angle, the open end of the gage may be held perpendicular to the horizon when it is dipped into the mercury in the ciſtern, without obliging us to bring our finger, or any other ſubſtance, into contact with the mercury in the gage, which never fails to render the inſtrument imperfect. It is neceſſary to obſerve, that if the tube be fourteen or fifteen inches long, I have never been able to boil it effectually for the experiments mentioned in this paper in leſs than three or four hours, although Mr. BROOK ſeems to preſcribe a much ſhorter time for the purpoſe; nor will it even then ſucceed, unleſs the greateſt attention be paid that no bubbles of air lurk behind, which to my own mortification I have frequently found to have been the caſe; but experience has at length taught me to guard pretty well againſt this diſappointment, particularly by taking care that the tube be completely dry before the mercury is put into it; for if this caution be not obſerved, the inſtrument can never be made perfect. There is, however, one evil which I have not yet been able to remedy; and that is, the introduction of air into the gage, owing to the unboiled mercury in the ciſtern; for when the gage has been a few times exhauſted, the mercury which originally filled it becomes mixed with that into which it is inverted, and in conſequence the vacuum is rendered leſs and leſs perfect, till at laſt the inſtrument is entirely ſpoiled. I have juſt conſtructed a gage ſo as to be able to boil the mercury in the ciſtern, but have not yet aſcertained its ſucceſs.

and

and was for ſome time diſpoſed to believe the contrary of what I am now convinced to be the truth. Indeed, if we reaſon *a priori*, I think we cannot ſuppoſe a perfect vacuum to be a perfect conductor without ſuppoſing an abſurdity: for if this were the caſe, either our atmoſphere muſt have long ago been deprived of all its electric fluid by being every where ſurrounded by a boundleſs conductor, or this fluid muſt pervade every part of infinite ſpace, and conſequently there can be no ſuch thing as a perfect vacuum in the univerſe. If, on the contrary, the truth of the preceding experiments be admitted, it will follow, that the conducting power of our atmoſphere increaſes only to a certain height, beyond which this power begins to diminiſh, till at laſt it entirely vaniſhes; but in what part of the upper regions of the air theſe limits are placed, I will not preſume to determine. It would not, perhaps, have been difficult to have applied the reſults of ſome of theſe experiments to the explanation of meteors, which are probably owing to an accumulation of electricity. It is not, however, my preſent deſign to give looſe to my imagination. I am ſenſible, that by indulging it too freely, much harm is done to real knowledge; and therefore, that one fact in philoſophy well aſcertained is more to be valued than whole volumes of ſpeculative hypotheſes.

Chatham-Place, Feb. 12, 1785.

Fig. 1.



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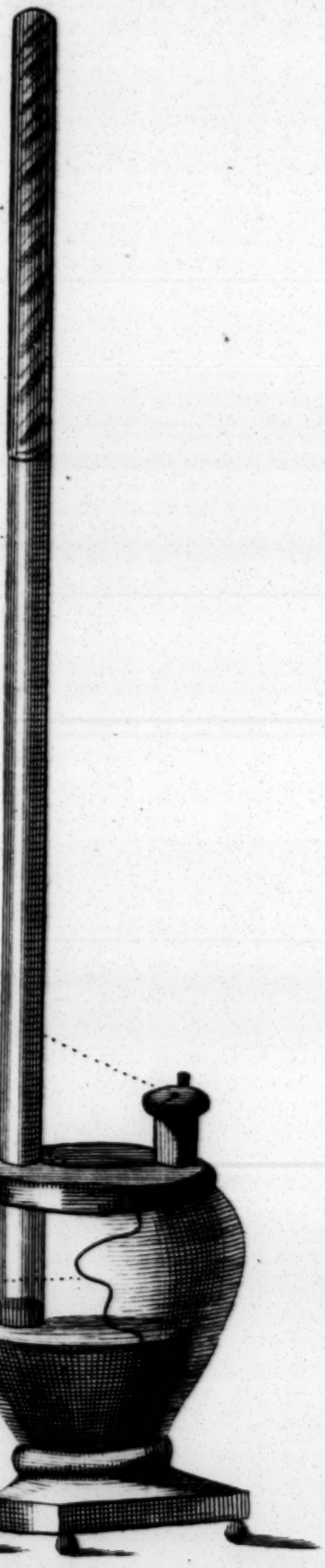
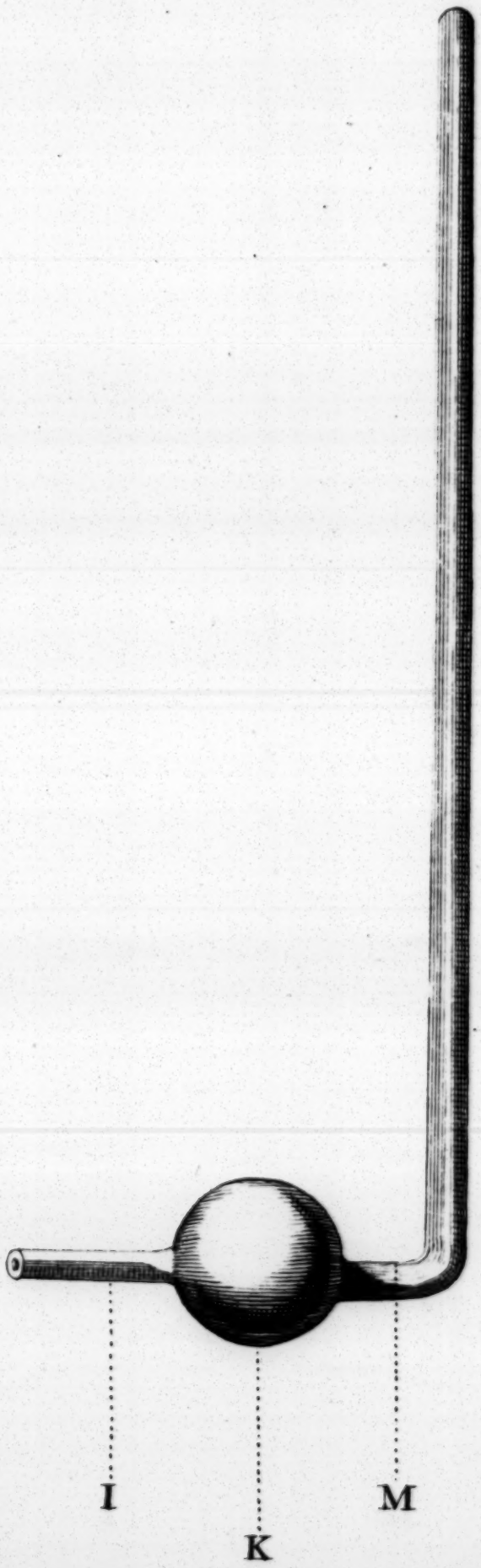


Fig. 2.



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XV.

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XV. *Experiments and Obſervations relating to Air and Water.*

By the Rev. Joſeph Prieſtley, *LL.D. F.R.S.*

Read February 24, 1785.

EVER ſince the diſcovery of the diminution of reſpirable air in thoſe proceſſes which are generally called *phlogiſtic*, it has been a great object with philoſophers to find what becomes of the air which diſappears in them. Among others, I have made and publiſhed a variety of experiments with that view; but though by this means ſome farther progreſs was made in the philoſophy of air, and conſequently our knowledge of the principles, or conſtituent parts, of natural ſubſtances was extended, I did not by any means ſucceed to my ſatisfaction with reſpect to the immediate object of my reſearches. Others, however, were more ſucceſsful, and their ſucceſs has at length enabled me to reſume my experiments with more advantage; by which means I have been led both to confirm their concluſions, and, by diverſifying the experiments, to throw conſiderable light upon various other chemical proceſſes. The reſult of theſe obſervations I ſhall lay before the Society, with as much brevity and diſtinctneſs as I can.

In the experiments of which I ſhall now give an account, I was principally guided by a view to the opinions which have lately been advanced by Mr. CAVENDISH, Mr. WATT, and M. LAVOISIER. Mr. CAVENDISH was of opinion, that when *air* is decompoſed, *water* only is produced; and Mr. WATT concluded

concluded from ſome experiments, of which I gave an account to the Society, and alſo from ſome obſervations of his own, that water conſiſts of dephlogiſticated and inflammable air, in which Mr. CAVENDISH and M. LAVOISIER concur with him; but Mr. LAVOISIER is well known to maintain, that there is no ſuch thing as what has been called *phlogiſton*, affirming inflammable air to be nothing elſe but one of the elements or conſtituent parts of water. In the following experiments I alſo had a particular view to a concluſion which I had drawn from thoſe experiments, of which an account is given in my laſt communications to the Royal Society; *viz.* that inflammable air is pure phlogiſton in the form of air, at leaſt with the element of *heat*; and that fixed air conſiſts of dephlogiſticated and inflammable air; both which doctrines had been firſt advanced by Mr. KIRWAN, before I had made the experiments which I then thought clearly proved them.

Such were the hypotheſes to which I had a view when I began the following courſe of experiments, which I hope will be an admonition to myſelf, as well as to others, to adhere as rigorouſly as poſſible to *actual obſervations*, and to be extremely careful not to overlook any circumſtance that may poſſibly contribute to any particular reſult. I ſhall have occaſion to notice my own miſtakes with reſpect to *concluſions*, though all the *facts* were ſtrictly as I have repreſented them. But whilſt philoſophers are faithful narrators of what they obſerve, no perſon can juſtly complain of being miſled by them; for to *reaſon* from the facts with which they are ſupplied is no more the province of the perſon who diſcovers them, than of him to whom they are diſcovered.

One of the moſt ſimple of all phlogiſtic proceſſes is that in which metals are ignited in dephlogiſticated air. I therefore

began with this, with a view to aſcertain whether any *water* is produced when the air is made to diſappear in it. Accordingly, into a glaſs veſſel containing 7 ounce meaſures of pretty pure dephlogiſticated air, I introduced a quantity of iron turnings (which is iron in ſmall thin pieces, exceedingly convenient for theſe and many other experiments) having previouſly made them, together with the veſſel, the air, and the mercury by which it was confined, as dry as I poſſibly could. Alſo, to prevent the air from imbibing any moiſture, I received it immediately in the veſſel in which the experiment was made, from the proceſs of procuring it from red precipitate; ſo that it had never been in contact with any water.

I then fired the iron, by means of a burning lens, and preſently reduced the 7 ounce meaſures of air to .65; but I found no more water after this proceſs than I imagined it had not been poſſible for me to exclude, as it bore no proportion to the air which had diſappeared. Examining the reſiduum of the air, I found one-fifth of it to be fixed air, and when I tried the purity of that which remained by the teſt of nitrous air, it did not appear that any phlogiſticated air had been produced in the proceſs: for though it was more impure than I ſuppoſe the air with which I began the experiment muſt have been, it was not more ſo than the phlogiſticated air of the 7 ounce meaſures, which had not been affected by the proceſs, and which muſt have been contained in the reſiduum, would neceſſarily make it. In this caſe one meaſure of this reſiduum and two of nitrous air occupied the ſpace of .32.

In another experiment of this kind, ten ounce meaſures of dephlogiſticated air were reduced to .8, and by waſhing in lime water to .38. In another experiment, in which $7\frac{1}{2}$ ounce meaſures of dephlogiſticated air were reduced to half an ounce

meaſure, of which one-fifth was fixed air, the reſiduum was quite as pure as the air with which I began the experiment, the teſt with nitrous air, in the proportions above-mentioned, giving .4 in both caſes. To what circumſtance the difference might be owing I cannot tell.

In theſe experiments the fixed air muſt, I preſume, have been formed by the union of the phlogiſton from the iron and the dephlogiſticated air in which it was ignited; but the quantity of it was very ſmall in proportion to the air which had diſappeared, and at that time I had no ſuſpicion that the iron, which had been melted, and gathered into round balls, could have imbibed it; a melting heat having been ſufficient, as I had imagined, to expel every thing that was capable of aſſuming the form of air from any ſubſtance whatever. I was therefore intirely at a loſs about what muſt have become of the air.

Senſible, however, that ſuch a quantity of air muſt have been imbibed by *ſomething* to which it muſt have given a very perceivable addition of weight, and ſeeing nothing elſe that could have imbibed it, it occurred to me to weigh the calx into which the iron had been reduced; and I preſently found, that the dephlogiſticated air had actually been imbibed by the melted iron, in the ſame manner as inflammable air, in my former experiments, had been imbibed by the melted calces of metals, however impoſſible ſuch an abſorption might have appeared to me *a priori*. In the firſt inſtance, about twelve ounce meaſures of dephlogiſticated air had diſappeared, and the iron had gained ſix grains in weight. Repeating the experiment very frequently, I always found, that other quantities of iron, treated in the ſame manner, gained ſimilar additions of weight, which was always very nearly that of the air which had diſappeared.

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This calx of iron, I then concluded, was by no means what I had before taken it to be, *viz.* a *pure calx* or *flag*, but either the calx, or the iron itſelf, ſaturated with pure air. This calciform ſubſtance I found, by various experiments, to be the ſame thing with the *ſcales* that fly from iron when it is made red-hot, or the ſubſtance into which it runs in a very intenſe heat, in an open fire.

Concluding from the preceding experiment, that iron, ſufficiently heated, was capable of ſaturating itſelf with pure air, extracted from the maſs of the atmoſphere, I then proceeded to melt it with the heat of a burning lens in the open air; and I preſently found, that perfect iron was eaſily fuſed in this way, and continued in this fuſion a certain time, exhibiting the appearance of *boiling* or *throwing out* air, whereas it was on the contrary *imbibing* air; and when it was ſaturated the fuſion ceaſed, and the heat of my lens could not make any farther impreſſion upon it. When this was the caſe, I always found that it had gained weight in the proportion of 7½ to 24, which is very nearly *one-third* of its original weight. The ſame was the effect when I melted *ſteel* in the ſame circumſtances, and alſo every kind of iron on which the experiment could be tried. But I have ſome reaſon to think, that with a greater degree of heat than I could apply, the iron might have been kept in a ſtate of fuſion ſomewhat longer, and by that means have imbibed more air, even more than one-third of its original weight.

There was a peculiar circumſtance attending the melting of *caſt iron* with a burning lens, which made it impoſſible to aſcertain the addition that was made to its weight, and at the ſame time afforded an amazing ſpectacle; for the moment that any quantity of it was melted, and gathered into a round ball, it began

to diſperſe in a thouſand directions, exhibiting the appearance of a moſt beautiful fire-work, ſome of the particles flying to the diſtance of half a yard from the place of fuſion; and the whole was attended with a conſiderable hiſſing noiſe. Some of the largeſt pieces which had been diſperſed in this manner I was able to collect, and having ſubjected them to the heat of the lens, they exhibited the ſame appearance as the larger maſs from which they had been ſcattered.

When I melted this caſt iron in the bottom of a deep glaſs receiver, in order to collect all the particles that were diſperſed, they firmly adhered to the glaſs, melting it ſuperficially, though without making it crack, ſo that it was ſtill impoſſible to collect and weigh the particles. However, I generally found that, notwithſtanding the copious diſperſion, what remained after the experiment rather exceeded than fell ſhort of the original weight of the iron. Sometimes a piece of common iron, and eſpecially ſteel, would make a little hiſſing in the fuſion, and a particle or two would fly off; but this was never conſiderable *.

Having now procured what I thought to be a new calx of iron, or a calx ſaturated with pure air, I endeavoured to revive it by making it imbibe inflammable air, in the ſame manner that I had before made iron, and various other metals, by melting them in a veſſel containing inflammable air. In this I ſucceeded; but in the courſe of the experiment a new and very unexpected appearance occurred. I took a piece of iron which I had ſaturated with pure air, and putting it into a glaſs veſſel

* On being informed of the above-mentioned phænomena, Mr. WATT concluded, that the baſis of the dephlogiſticated air united to the phlogiſton of the iron, and formed *water*, which was attracted by, and remained ſo firmly united to the calx of iron, as to reſiſt the effects of heat to ſeparate them.

containing

containing inflammable air, confined by water, threw upon it the focus of the lens, and preſently perceived the inflammable air to diſappear, and without thinking of any thing eſcaping from the calx of iron (which had been ſubjected to a greater heat before) I imagined that I ſhould have found the addition of the weight of air in the iron, and the reſult might be an iron different from the common ſort. But I found, to my ſurpriſe, that the iron, which had exhibited no new appearance in this mode of treatment, had loſt weight, inſtead of gaining any. The piece of iron on which I made this firſt experiment weighed 11½ grains, and 7½ ounce meaſures of inflammable air had diſappeared while the iron had loſt 2½ grains.

Conſidering the quantity of inflammable air that had diſappeared, *viz.* 7½ ounce meaſures, and the dephlogiſticated air which had been expelled from the iron, *viz.* 2½ grains, which is equal to about 4.1 ounce meaſures, I found that they were very nearly in the proper proportion to ſaturate each other, when decompoſed by the electrical ſpark, *viz.* two meaſures of inflammable air to one of dephlogiſticated air. I therefore had now no doubt but that the two kinds of air had united, and had formed either *fixed air* or *water*; but which it was I could not tell, having had water on the receiver in which the experiment was made, and having neglected to examine the ſtate of the air that remained, except in a general way, by which I found, that it was ſtill, to appearance, as inflammable as ever.

With a view to determine whether *fixed air*, or *water*, would be the produce of this mode of combining inflammable and dephlogiſticated air, I repeated the experiment in a veſſel in which the inflammable air was confined by mercury, and both the veſſel and the mercury had been previouſly made as dry as poſſible. I had no ſooner begun to heat the iron, or rather *ſlag*,

in theſe circumſtances, than I perceived the air to diminiſh, and at the ſame time the inſide of the veſſel to grow very cloudy, with particles of dew, that covered almoſt the whole of it. Theſe particles by degrees gathered into drops, and ran down the ſides of the veſſel in all places, except where it was heated by the ſun-beams; ſo that it then appeared to me very evident, that *water*, with or without fixed air, was the produce of the inflammable air, and the pure air let looſe from the iron in this mode of operation; though afterwards I was taught by Mr. WATT to correct this hypotheſis, and to account for this reſult in a different manner. When I had examined the remaining air, it was as inflammable as ever, without containing any mixture of fixed air at all.

When I collected the water which was produced in this experiment by means of a piece of filtering paper, carefully introduced to abſorb it, I found it to be, as nearly as poſſible, of the ſame weight with that which had been loſt by the iron: and alſo, in every experiment of this kind, in which I attended to this circumſtance, I found that the quantity of inflammable air which had diſappeared was about double to that of the dephlogiſticated air ſet looſe from the iron, ſuppoſing that weight to have been reduced into air. Thus at one time I made a piece of this ſlag imbibe 5½ ounce meaſures of inflammable air, while it loſt as much as the weight of about 3 ounce meaſures of dephlogiſticated air, and the water collected weighed 2 grains. Another time a piece of ſlag loſt 1.5 grains, and the water produced was 1.7 grains; but perfect accuracy is not to be expected. I ſhall only mention one more experiment of this kind, in which 6½ ounce meaſures of inflammable air were reduced to .92 ounce meaſure, and the iron had loſt 2 grains, equal in weight to 3.3 ounce meaſures of dephlogiſticated air.

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In all the above-mentioned experiments, the inflammable air was that which is produced by the ſolution of iron in acids.

As before I had finiſhed this courſe of experiments I had ſatisfied myſelf that inflammable air always contains a portion of water; and alſo, that when it has been ſome time confined by water, it imbibes more, ſo as to be increaſed in its ſpecific gravity by that means, I repeated the experiment with inflammable air which had not been confined by water, but which was received in a veſſel of dry mercury from the veſſel in which it was generated; but I preſently perceived that water was produced in this caſe alſo, and to appearance as copiouſly as in the former experiment. Indeed, the quantity of water produced, which ſo greatly exceeded the weight of all the inflammable air, is ſufficient to prove that it muſt have had ſome other ſource than any conſtituent part of that air, or the whole of it, together with the water contained in it, without taking into conſideration the correſponding loſs of weight in the iron.

I muſt here obſerve, that the iron ſlag which I had treated in this manner, and which had thereby loſt the weight which it had acquired by melting in dephlogiſticated air, became *perfect iron* as at firſt, and was then capable of being melted by the burning lens again; ſo that the ſame piece of iron would ſerve for theſe experiments as long as the operator ſhould chuſe. It was evident, therefore, that if the iron had loſt its phlogiſton in the preceding fuſion, it had acquired it again from the inflammable air which it had abſorbed; and I do not ſee how the experiment can be accounted for in any other way, which neceſſarily implies the reality of phlogiſton as a conſtituent principle in bodies. This, at leaſt, is the moſt natural way of accounting for the appearances.

Having had this ſucceſs with the calx, or ſcales of *iron*, I tried the calx of *copper*, or thoſe ſcales which fly from it when it is made red-hot; and I found water produced in the inflammable air in the ſame manner as when I uſed the ſcales of iron in the ſame circumſtances. I alſo had the ſame reſult when I revived *precipitate per ſe* in inflammable air; but having at that time a very weak winter's ſun, I could not make the experiment with ſo much advantage as I could have wiſhed.

Iron, I found, acquired this additional weight by melting in an earthen retort, as well as in the open air by the ſunbeams, if it were poſſible for it to attract air, or whatever elſe it is that is the immediate cauſe of its additional weight. Three ounces of common iron filings, expoſed to a ſtrong heat in an earthen retort, gained 11 dwts, or 264 grains, and yet was very far from having been completely fuſed. Having a glaſs tube communicating with the retort, in order to collect any air that the iron filings might give out, I found that when they were very hot, the water aſcended within the tube; which ſhews that the iron was then in a ſtate of abſorbing, and not of giving out any air.

Seeing ſo much water produced in theſe experiments with inflammable air, I was particularly led to reflect on the relation which they bore to each other, and eſpecially to Mr. CAVENDISH's ideas on the ſubject. He had told me that, notwithſtanding the experiments of which I had given an account to the Royal Society, and from which I had concluded that inflammable air was pure phlogiſton, he was perſuaded that *water* was eſſential to the production of it, and even entered into it as a conſtituent principle. At that time I did not perceive the force of the arguments which he ſtated to me, eſpecially as, in the experiments with charcoal, I totally diſperſed any quantity

of

of it with a burning lens *in vacuo*, and thereby filled my receiver with nothing but inflammable air. I had no ſuſpicion that the wet leather on which my receiver ſtood could have any influence in the caſe, while the piece of charcoal was ſubject to the intenſe heat of the lens, and placed ſeveral inches above the leather. I had alſo procured inflammable air from charcoal in a glazed earthen retort two whole days ſucceſſively, in which it had given inflammable air without intermiſſion. Alſo iron filings in a gun-barrel, and a gun-barrel itſelf, had always given inflammable air whenever I tried the experiment.

Theſe circumſtances, however, deceived me, and perhaps would have deceived any other perſon; for I did not know, and could not have believed, the powerful attraction that *charcoal*, or *iron*, appear to have for *water* when they are intenſely hot. They will find, and attract it, in the midſt of the hotteſt fire, and through any pores that may be left open in a retort; and iron filings are ſeldom ſo dry as not to have moiſture enough adhering to them, capable of enabling them to give a conſiderable quantity of inflammable air. But my attention being now fully awake to the ſubject, I preſently found that the circumſtances above-mentioned had actually miſled me; I mean with reſpect to the *concluſion* which I drew from the experiments, and not with reſpect to the experiments themſelves, every one of which, I doubt not, will be found to anſwer, whenever they are tried by perſons of ſufficient ſkill and properly attentive to all the circumſtances.

Being thus appriſed of the influence of unperceived moiſture in the production of inflammable air, and willing to aſcertain it to my perfect ſatisfaction, I began with filling a gun-barrel with iron filings in their common ſtate, without taking any particular precaution to dry them, and I found that they gave air as

they had been uſed to do, and continued to do ſo many hours; I even got ten ounce meaſures of inflammable air from two ounces of iron filings in a coated glaſs retort. At length, however, the production of inflammable air from the gun-barrel ceaſed; but on putting water into it, the air was produced again, and a few repetitions of the experiment fully ſatisfied me that I had been too precipitate in concluding that inflammable air is pure phlogiſton.

I then repeated the experiment with the charcoal, making the receiver, the ſtand on which I placed the charcoal, and the charcoal itſelf, as dry and as hot as poſſible, and uſing cement inſtead of a wet leather to exclude the air. In theſe circumſtances I was not able, with the advantage of a good ſun, and an excellent burning lens, to decompoſe quite ſo much as two grains of the piece of charcoal, which gave me ten ounce meaſures of inflammable air; and this, I imagine, was effected by means of ſo much moiſture as was depoſited from the air in its ſtate of rarefaction, and before it could be drawn from the receiver. To the production of this kind of inflammable air I was therefore now convinced, that water is as neceſſary as to that from iron.

It was in this ſtate of my experiments that I received an authentic account of thoſe of M. LAVOISIER, on tranſmitting water through an hot iron tube and alſo through a hot copper tube containing charcoal, and thereby procuring large quantities of inflammable air, M. LAVOISIER himſelf having been ſo obliging as to ſend me a copy of his Memoir on that ſubject. I had heard an account of the experiments ſome months before; but it was ſo imperfect a one, that I own I paid little attention to them. At this time, however, I was prepared to be ſufficiently ſenſible of their value.

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In my laſt communications to the Royal Society, it will be ſeen that I had tranſmitted the vapour of ſeveral fluid ſubſtances through red-hot *earthen tubes*, and thereby procured different kinds of air. M. LAVOISIER adopted the ſame proceſs, but uſed an *iron tube*; and by means of that circumſtance made a very valuable diſcovery which had eſcaped me. I had indeed on one occaſion made uſe of an iron tube, and tranſmitted ſteam through it; but not having at that time any view to the production of *air*, I did not collect it at all, contenting myſelf with obſerving that *water*, after being made red-hot, was ſtill water, there being no change in its ſenſible properties. Being now farther inſtructed by the experiment of M. LAVOISIER, I was determined to repeat the proceſs with all the attention I could give to it; but I ſhould not have done this with ſo much advantage, if I had not had the aſſiſtance of Mr. WATT, who always thought that M. LAVOISIER's experiments by no means favoured the concluſion that he drew from them. As to myſelf, I was a long time of opinion that his concluſion was juſt, and that the inflammable air was really furniſhed by the water being decompoſed in the proceſs. But though I continued to be of this opinion for ſome time, the frequent repetition of the experiments, with the light which Mr. WATT's obſervations threw upon them, ſatisfied me at length that the inflammable air came principally from the charcoal, or the iron.

I ſhall firſt relate the reſult of the experiment that was made with *charcoal*, and then thoſe with iron and other ſubſtances, in contact with which (when they were in a ſtate of fuſion, or at leaſt red-hot) I made ſteam, or the vapour of other liquid ſubſtances, to paſs. I ſhall only obſerve that, previous to this, I began to make the experiments with coated glaſs tubes, which

I found to anſwer very well during the proceſs, though they never failed to break in cooling. At length I procured a tube of *copper*, on which, as M. LAVOISIER diſcovered, ſteam had no effect; and at laſt I made uſe of earthen tubes, with which Mr. WEDGEWOOD, that moſt generous promoter of ſcience, liberally ſupplied me for the purpoſe; and theſe glazed on the outſide only I find far preferable to copper. They are, indeed, every thing that I could wiſh for in experiments of this kind; the reaſon of which will appear in my account of another courſe of experiments, which I hope to lay before the Society in due time.

The diſpoſition of the apparatus, with which theſe experiments were made, was as follows. The water was made to boil in a glaſs retort, which communicated with the copper or earthen tube which contained the charcoal or iron, &c. and which, being placed in an horizontal poſition, was ſurrounded with hot coals. The end of this tube oppoſite to the retort communicated with the pipe of a common *worm tub*, ſuch as is generally uſed in diſtillations, by means of which all the ſuperfluous ſteam was condenſed, and collected in a proper receptacle, while the air which had been produced, and had come along with it through the worm tub, was tranſmitted into a trough of water, where proper veſſels were placed to receive it, and aſcertain the quantity of it; after which I could examine the quality of it at leiſure.

In the experiment with *charcoal*, I found unexpected difficulties, and conſiderable variations in the reſult; the proportion between the *charcoal* and *water* expended, and alſo between each ofth em and the *air* produced, not being ſo nearly the ſame as I imagined they would have been. Alſo the quantity of fixed air that was mixed with the inflammable air varied very much.

much. This laſt circumſtance, however, ſome of my experiments may ſerve to explain. Whenever I had no more water than was ſufficient for the production of the air, there was never any ſenſible quantity of uncombined fixed air mixed with the inflammable air from charcoal. This was particularly the caſe when I produced the air by means of a burning lens in an exhauſted receiver, and alſo in an earthen retort with the application of an intenſe heat. I therefore preſume, that when the ſteam tranſmitted through the hot tube containing the charcoal was very copious, the fixed air in the produce was greater than it would otherwiſe have been. The extremes that I have obſerved in the proportion of the fixed to the inflammable air have been from one-twelfth to one-fifth of the whole. As I generally produced this air, the latter was the uſual proportion; and this was excluſive of the fixed air that was intimately combined with the inflammable air, and which could not be ſeparated from it except by decompoſition with dephlogiſticated air; and this combined fixed air I ſometimes found to be one-third of the whole maſs, though at other times not quite ſo much.

To aſcertain this, I mixed one meaſure of this inflammable air from charcoal (after the uncombined fixed air had been ſeparated from it by lime-water) with one meaſure of dephlogiſticated air, and then fired them by the electric ſpark. After this I always found that the air which remained made lime-water very turbid, and the proportion in which it was now diminiſhed, by waſhing in lime-water, ſhewed the quantity of fixed air that had been combined with the inflammable. That the fixed air is not *generated* in this proceſs, is evident from there being no fixed air found after the exploſion of dephlogiſticated air and inflammable air from iron.

Notwith-

Notwithſtanding the above-mentioned variations, the loſs of weight in the charcoal was always much exceeded by the weight of the water expended, which was generally more than double of the charcoal; and this water was intimately combined with the air; for when I received a portion of it in mercury, no water was ever depoſited from it.

The experiment which, upon the whole, gave me the moſt ſatisfaction, and the particulars of which I ſhall therefore recite, was the following. Expending 94 grains of perfect charcoal (by which I mean charcoal made with a very ſtrong heat, ſo as to expel all fixed air from it) and 240 grains of water, I procured 840 ounce meaſures of air, one-fifth of which was fixed air, and of the inflammable part nearly one-third more appeared to be fixed air by decompoſition.

Receiving this kind of air in a variety of experiments, but not in the preceding ones in particular (for then I could not have aſcertained the quantity of it) conſiſting of fixed and inflammable air together, I found ſome variations in its ſpecific gravity, owing, I imagine, to the different proportions of fixed air contained in it; but upon the whole, I think, that the proportion of 14 grains to 40 ounce meaſures is pretty near the truth, when the proportion of fixed air is about one-fifth of the whole. With reſpect to the weight of the inflammable air after the fixed air was ſeparated from it, I found no great difference, and think it may be eſtimated at 8 grains to 30 ounce meaſures.

Upon theſe principles, the whole weight of the 840 ounce meaſures of air will be - 294 grains

that of the charcoal will be 94
that of the water - 240

334 which, conſidering the na-

ture of the experiment, will perhaps be thought to be tolerably near that of the air.

If the air be analyzed, the 840 ounce meaſures will be found to contain - 168 of uncombined fixed air = 151 grains.
and 672 impure imflammable = 179
ſo that the whole 840 will weigh - - 330

Laſtly, if the 672 ounce meaſures of impure inflammable air be decompoſed, it will be found to contain
164 ounce meaſures of fixed air = 147.6 grs.
and 508 inflammable - - = 30.7
ſo that the whole 672 will weigh - - 178.3
which is very near to 179, the weight of the whole together.

It may, however, be ſafely concluded from this experiment, and indeed from every other that I made with charcoal, that there was no more pure inflammable air produced than the charcoal itſelf may be very well ſuppoſed to have ſupplied.

There is, therefore, no reaſon for deſerting the old eſtabliſhed hypotheſis of *phlogiſton* on account of theſe experiments, ſince the fact is by no means inconſiſtent with it. The pure inflammable air with the water neceſſarily contained in it would weigh no more than about 30 grains, while the loſs of weight in the charcoal was 94 grains. But to this muſt be added the phlogiſton contained in 392 ounce meaſures of fixed air, which, according to Mr. KIRWAN's proportion, will be nearly 65 grains, and this and the 30 grains will be 95 grains.

The baſis to this fixed air, as well as to the inflammable, muſt have been furniſhed by the *water*; and from this it may be concluded, that the water muſt have been ſo far altered as to be changed into fixed air, which will be thought not to be any great paradox, if it be conſidered that, according to the

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lateſt diſcoveries, fixed air and water appear to conſiſt of the ſame ingredients, namely dephlogiſticated and inflammable air. However, in this change of the water we cannot be abſolutely ſure that the ſame proportion of the ingredients is contained, and therefore it cannot be abſolutely determined whether the inflammable air which it contains enters wholly into the fixed air, or not. Farther experiments, or a careful compariſon of theſe experiments with thoſe made by Mr. KIRWAN and others, may perhaps throw ſome light upon this ſubject. Whether the combined fixed air comes wholly from the charcoal, or whether the charcoal only ſupplies the phlogiſton, and the water its baſis, that is, the dephlogiſticated air, deſerves to be inveſtigated.

Before I conclude my account of the experiments with charcoal, I would obſerve, that there is another in which I place ſome dependance, in which, with the loſs of 178 grains of charcoal, and 528 grains of water, I procured 1410 ounce meaſures of air, of which the laſt portion (for I did not examine the reſt) contained one-ſixth part of uncombined fixed air. This was made in an earthen tube glazed on the outſide.

The experiments with *iron* were more ſatisfactory than thoſe with charcoal, being ſubject to leſs variation; and it is ſtill more evident from them, that the inflammable air does not come from the *water*, but only from the *iron*, as the quantity of water expended, added to the weight of the air produced, was as nearly as could be expected in experiments of this kind, found in the addition of weight gained by the iron. And though the inflammable air procured in this proceſs is between one-third and one-half more than can be procured from iron by a ſolution in acids, the reaſon may be, that much phlogiſton is retained in the ſolutions, and therefore much more may be expelled from iron, when pure water, without any acid, takes the place of it. I would further obſerve, that the produce of

air,

air, and alſo the addition of weight gained by the iron, are much more eaſily aſcertained in theſe experiments than the quantity of water expended in them, on account of the great length of the veſſels uſed in the proceſs, and the different quantities that may perhaps be retained in the worm of the tub, though I did not fail to uſe all the precautions that I could think of to guard againſt any variation on theſe accounts.

Of the many experiments which I made with *iron*, I ſhall content myſelf with reciting the following reſults. With the addition of 267 grains to a quantity of iron, and the loſs of 336 grains of water, I procured 840 ounce meaſures of inflammable air; and with the addition of 140 grains to another quantity of iron, and the conſumption of 253 grains of water, I got 420 ounce meaſures of air *.

The inflammable air produced in this manner is of the lighteſt kind, and free from that very *offenſive ſmell* which is generally occaſioned by the rapid ſolution of metals in oil of vitriol, and it is extricated in as little time in this way as it is poſſible to do it by any mode of ſolution. On this account it occurred to me, that it muſt be by much the cheapeſt method that has yet been uſed of filling *balloons* with the lighteſt inflammable air. For this purpoſe it will be proper to make uſe of caſt-iron cylinders of a conſiderable length, and about three or four inches, or perhaps more, in diameter. Though the iron tube itſelf will contribute to the production of air, and therefore may become unfit for the purpoſe in time; yet, for any

* If the perfect accuracy of the former of theſe experiments may be depended on (and it may always be preſumed, that thoſe in which *little water* is expended are preferable to thoſe in which *more* is conſumed) the *water* that neceſſarily enters into this kind of inflammable air is about equal in weight to the *phlogiſton* that is in it. But I propoſe to give more particular attention to this ſubject.

thing that I know to the contrary, the ſame tube may ſerve for a very great number of proceſſes, and perhaps the change made in the inſide ſurface may protect it from any farther action of the water, if the tube be of ſufficient thickneſs; but this can only be determined by experiment.

Some eſtimate of what may be expected from this method of procuring inflammable air may be formed from the following obſervations. About twelve inches in length of a copper tube, three-fourths of an inch in diameter, filled with *iron turnings* (which are more convenient for this purpoſe than *iron filings*, as they do not lie ſo cloſe, but admit the ſteam to paſs through their interſtices) when it was heated, and a ſufficient quantity of ſteam paſſed through it, yielded thirty ounce meaſures of air in fifty ſeconds; and eighteen inches of another copper tube, an inch and a quarter in diameter, filled and treated in the ſame manner, gave two hundred ounce meaſures in one minute and twenty-five ſeconds; ſo that this larger tube gave air in proportion to its ſolid contents compared with the ſmaller; but to what extent this might be depended upon I cannot tell. However, as the heat penetrates ſo readily to ſome diſtance, the rate of giving air will always be in a greater proportion than that of the ſimple diameter of the tube.

The following experiment was made with a view to aſcertain the quantity of inflammable air that may be procured in this way from any given quantity of iron. Two ounces of iron, or 960 grains, when diſſolved in acids, will yield about 800 ounce meaſures of air; but treated in this manner it yielded 1054 ounce meaſures, and then the iron had gained 329 grains in weight, which is little ſhort of one-third of the weight of the iron.

Conſidering

Conſidering how little this inflammable air weighs, *viz.* the whole 1054 ounce meaſures not more than 63 grains, and the difficulty of aſcertaining the loſs of water to ſo ſmall a quantity as this, it is not poſſible to determine, from a proceſs of this kind, how much water enters into the compoſition of the inflammable air of metals. It would be more eaſy to determine this circumſtance with reſpect to the inflammable air of charcoal, eſpecially by means of the experiment made with a burning lens *in vacuo*. In this method two grains of charcoal gave at a medium thirteen ounce meaſures of inflammable air, which, in the proportion of 30 ounce meaſures to 8 grains, will weigh 3.3 grains; ſo that water in the compoſition of this kind of inflammable air is in the proportion of 1.3 to 2, though there will be ſome difficulty with reſpect to the fixed air intimately combined with this kind of inflammable air.

Since iron gains the ſame addition of weight by melting in *dephlogiſticated air*, and alſo by the addition of *water* when red-hot, and becomes, as I have already obſerved, in all reſpects the ſame ſubſtance, it is evident, that this *air* or *water*, as exiſting in the iron, is the very ſame thing; and this can hardly be explained but upon the ſuppoſition that water conſiſts of two kinds of air; *viz.* inflammable and dephlogiſticated. I ſhall endeavour to explain theſe proceſſes in the following manner.

When iron is melted in dephlogiſticated air, we may ſuppoſe that, though part of its phlogiſton eſcapes, to enter into the compoſition of the ſmall quantity of fixed air which is then procured, yet enough remains to form *water* with the addition of dephlogiſticated air which it has imbibed, ſo that this *calx* of iron conſiſts of the intimate union of the pure *earth of iron* and of *water*; and therefore when the ſame calx, thus ſatu-

rated with water, is expoſed to heat in inflammable air, this air enters into it, deſtroys the attraction between the water and the earth, and revives the iron, while the water is expelled in its proper form.

Conſequently, in the proceſs with *ſteam*, nothing is neceſſary to be ſuppoſed but the entrance of the water, and the expulſion of the phlogiſton belonging to the iron, no more phlogiſton remaining in it than what the water brought along with it, and which is retained as a conſtituent part of the water, or of the new compound.

Having procured water from the ſcales of iron (which I muſt again obſerve is, in all reſpects, the ſame ſubſtance with iron melted in dephlogiſticated air, or ſaturated with ſteam by means of heat) and having thereby converted it into perfect iron again, I did not entertain a doubt but that I ſhould be able to produce the ſame effect by heating it with charcoal in a retort; and I had likewiſe no doubt but I ſhould be able to extract the additional weight which the iron had gained (*viz.* one-third of the whole) in *water*. In the former of theſe conjectures I was right; but with reſpect to the latter, I was totally miſtaken.

Having made the ſcales of iron, and alſo the powder of charcoal very hot, previous to the experiment, ſo that I was ſatisfied that no air could be extracted from either of them ſeparately by any degree of heat, and having mixed them together while they were hot, I put them into an earthen retort, glazed within and without, which was quite impervious to air. This I placed in a furnace, in which I could give it a very ſtrong heat; and connected with it proper veſſels to condenſe and collect the water which I expected to receive in the courſe of the proceſs. But, to my great ſurpriſe, not one particle

of

of *moiſture* came over, but a prodigious quantity of *air*, and the rapidity of its production aſtoniſhed me; ſo that I had no doubt but that the weight of the air would have been equal to the loſs of weight both in the ſcales and in the charcoal; and when I examined the air, which I repeatedly did, I found it to contain one-tenth of fixed air, and the inflammable air, which remained when the fixed air was ſeparated from it, was of a very remarkable kind, being quite as heavy as common air. The reaſon of this was ſufficiently apparent when it was decompoſed by means of dephlogiſticated air; for the greateſt part of it was fixed air.

The theory of this proceſs I imagine to be, that the phlogiſton from the charcoal reviving the iron, the water with which it had been ſaturated, being now ſet looſe, affected the hot charcoal as it would have done if it had been applied to it in the form of *ſteam* as in the preceding experiments; and therefore the air produced in theſe two different modes have a near reſemblance to each other, each containing fixed air, both combined and uncombined, though in different proportions; and in both the caſes I found theſe proportions ſubject to variations. In one proceſs with charcoal and ſcales of iron, the firſt produce contained one-fifth of uncombined fixed air, the middle part one-tenth, and the laſt none at all. But in all theſe caſes the proportion of combined fixed air varied very little.

Why *air* and not *water* ſhould be produced in this caſe, as well as in the preceding, when the iron is equally revived in both, I do not pretend perfectly to underſtand. There is, indeed, an obvious difference in the circumſtances of the two experiments; as in that with charcoal the phlogiſton is found in a combined ſtate; whereas in that of inflammable air, it is

looſe,

looſe, or only united to water; and perhaps future experiments may diſcover the operation of this circumſtance.

There is ſome analogy between the experiment of the calx of iron imbibing inflammable air, and the iron itſelf imbibing dephlogiſticated air. In the former caſe *water* is produced, and in the latter *fixed air*. However, this caſe of iron imbibing dephlogiſticated air more nearly reſembles the caſe of the *blood in the lungs* imbibing the ſame kind of air, and in both the caſes as dephlogiſticated air is imbibed, fixed air is formed. This, therefore, ſeems to be a confirmation of the concluſion which I drew from my former experiments on blood, *viz.* that it parts with phlogiſton in reſpiration. Only I would now add, that at the ſame time that it parts with phlogiſton it takes in dephlogiſticated air, which makes the caſe perfectly ſimilar to that of the experiment with *iron*, which likewiſe parts with phlogiſton to form fixed air, at the ſame time that it imbibes dephlogiſticated air in contact with which it is fuſed.

I propoſe to reſerve for a future communication the continuation of theſe experiments, containing an account of the application of the ſame proceſs to other ſubſtances; but it may not be amiſs juſt to mention a few of the *general reſults*, and thoſe which have the neareſt connexion with the experiments recited above.

After having tranſmitted ſteam in contact with *charcoal* and *iron* in a copper tube, I propoſed to do the ſame with other ſubſtances containing phlogiſton, and I began with *bones*, which were burnt black, and had been ſubjected to an intenſe heat, covered with ſand, in an earthen retort. From three ounces of bone thus prepared, and treated as I had done the charcoal, I got 840 ounce meaſures of air, with the loſs of 288 grains of water. The bones were by this means made perfectly white,

and had loſt 110 grains of their weight. As the air ceaſed to come a conſiderable time before all the water had been tranſmitted through the tube containing them, I concluded that the air was formed from the phlogiſton contained in the bones, and ſo much water as was neceſſary to give it the form of air.

This air differs conſiderably from any other kind of inflammable air, being in ſeveral reſpects a medium between that from charcoal and that from iron. It contains about one-fourth of its bulk of uncombined fixed air, but not quite one-tenth intimately combined with the remainder. The water that came over was blue, and pretty ſtrongly alkaline, which muſt have been occaſioned by the volatile alkali not having been intirely expelled from the bones in the former proceſs, and its having in part diſſolved the copper of the tube in which the experiment was made.

I ſubjected to the ſame proceſs a variety of ſubſtances that are ſaid not to contain phlogiſton, but I was never able to procure inflammable air by means of them; which ſtrengthens the hypotheſis of the principal element in the conſtitution of this air having been derived from the ſubſtance ſuppoſed to contain phlogiſton, and therefore that phlogiſton is a real ſubſtance, capable of aſſuming the form of air by means of water and heat.

The experiments above-mentioned relating to iron were made with that kind which is *malleable*; but I had the ſame reſult when I made uſe of ſmall nails of *caſt iron*, except that theſe were firmly faſtened together after the experiment, the ſurfaces of them being cryſtallized, and the cryſtals mixing with each other, ſo that it was with great difficulty that they could be got out of the tube after the experiment, and in general the ſolid parts of the nails were broken before they were ſeparated from

from each other. Indeed the pieces of malleable iron adhered together after the experiment, but by no means ſo firmly.

Caſt iron annealed (by being kept red-hot in charcoal) is remarkably different from the caſt iron which has not undergone that operation, eſpecially in its being, to an extraordinary degree, more ſoluble in acids. With the turnings of annealed caſt iron I made the following experiment. From 960 grains of this iron, and with the loſs of 480 grains of water, I got 870 ounce meaſures of inflammable air, and tranſmitting ſteam through them a ſecond time, I got 150 ounce meaſures more. The iron had then gained 246 grains in weight, and the pieces adhered firmly together; but being thin they were eaſily broken and got out of the tube, whereas it had required a long time, and a ſharp ſteel inſtrument, to clear the tube of the caſt-iron nails.

Having got water from the ſcales of iron and of copper ſaturated with dephlogiſticated air, by heating them in inflammable air, it occurred to me to make the ſame experiment with *precipitate per ſe*, and I found, that the moment that the focus of the lens fell upon this ſubſtance the mercury began to revive, the inflammable air rapidly diſappeared, and *water* was formed on the ſides of the veſſel in which the experiment was made. For want of a better ſun, I could not aſcertain every circumſtance relating to this proceſs; but what I did ſeemed to afford a ſufficient proof that mercury contains phlogiſton, and that it is not revived by the mere expulſion of dephlogiſticated air, as M. LAVOISIER ſuppoſes; eſpecially as *no fixed air* was found in what remained of the inflammable air. In one of theſe experiments 4.5 ounce meaſures of inflammable air had diſappeared, and 1.6 ounce meaſure remained; and this appeared to contain ſome dephlogiſticated air mixed with the inflammable.

Willing to try the effect of heating iron, and other ſubſtances, in all the different kinds of air, without any particular expectation, I found that iron melted more readily in *vitriolic acid* air than in dephlogiſticated air, the air was diminiſhed as rapidly, and the inſide of the veſſel was covered with a *black ſooty matter*, which, when expoſed to heat, readily ſublimed in the form of a white vapour, and left the glaſs quite clean. The iron, after the experiment, was quite brittle, and muſt, I preſume, be the ſame thing with iron that is *ſulphurated*; but I did not particularly examine it. Of ſeven ounce meaſures of vitriolic acid air, in one of theſe experiments, not more than three-tenths of an ounce meaſure remained; of this two-thirds was fixed air, and the reſiduum of this was inflammable. I had put three of ſuch reſiduums together, in order to make the experiment with the greater certainty.

Having tranſmitted *ſteam*, or the vapour of water, through a copper tube, I was willing to try the effects of *ſpirit of wine* through the ſame tube when red-hot, having before procured inflammable air by ſending the ſame vapour through a red-hot tobacco-pipe. In this caſe, the vapour of the ſpirit of wine had no ſooner entered the hot copper tube, than I was perfectly aſtoniſhed at the rapid production of air. It reſembled the blowing of a pair of bellows. But I had not uſed four ounces of the ſpirit of wine before I very unexpectedly found, that the tube was perforated in ſeveral places; and preſently afterwards it was ſo far deſtroyed, that in attempting to remove it from the fire it actually fell in pieces. The inſide was full of a black ſooty matter reſembling lamp-black.

Upon this I had recourſe to *earthen tubes*, and found, that by melting copper and other metals in them, and tranſmitting the

vapour of ſpirit of wine in contact with them, different ſubſtances were formed according to the metals employed. The new ſubſtances hereby formed may be ſaid to be the ſeveral metals ſuper-ſaturated with phlogiſton, and may perhaps not be improperly called the *charcoal of metals*.

That this appellation is not very improper, may appear from theſe ſubſtances yielding inflammable air very copiouſly when they are made red-hot, and the ſteam of *water* is tranſmitted in contact with them, juſt as when the charcoal of wood is treated in the ſame manner. The detail of theſe experiments I reſerve for another communication, as alſo thoſe of the converſion of *ſpirit of wine*, *æther*, and *oil*, into different kinds of inflammable air, by tranſmitting them, in vapour, through hot earthen tubes. In the mean time, I ſhall think myſelf happy if the communication of the preceding experiments ſhall give any ſatisfaction to the Members of the Society.

POSTSCRIPT.

BEFORE I cloſe this paper, I wiſh to make a few *general inferences* from the principal of the experiments above-mentioned, eſpecially relating to the proportional quantity of phlogiſton contained in *iron* and *water*.

When any quantity of iron is melted in dephlogiſticated air, it imbibes the greateſt part of it, and gains an addition of weight very nearly equal to that of the air imbibed. Thus the abſorption of twelve ounce meaſures of dephlogiſticated air

gave an addition of ſix grains to the piece of iron which had been melted in it. But there was always a quantity of *fixed air* produced in this proceſs; and on the ſuppoſition that this air conſiſts of the union of dephlogiſticated and inflammable air, it proves that the dephlogiſticated air which enters the iron expels more phlogiſton than is neceſſary to conſtitute an equal weight of water, ſo that *water* does not contain ſo much phlogiſton as *iron*; but the difference is not very conſiderable.

Admitting Mr. KIRWAN's concluſion, *viz.* that 100 cubic inches of fixed air contain 8,357 grains of phlogiſton, the .13 ounce meaſure of fixed air, which (in an experiment recited in theſe papers) was found in the reſiduum of ſeven ounce meaſures of dephlogiſticated air abſorbed by iron, would not have contained more than .01 grain of phlogiſton, or about .16 ounce meaſure of inflammable air. Then, as the abſorption of 12 ounce meaſures of dephlogiſticated air occaſioned an addition of 6 grains to the weight of the iron which had abſorbed it, the abſorption of ſeven ounce meaſures muſt have occaſioned the addition of 3.5 grains to the iron which had imbibed it. But the ſame addition of weight to iron given by *ſteam* (which carries its own inflammable air along with it) would have expelled near 12 ounce meaſures of inflammable air: conſequently, about ten ounce meaſures of inflammable air (or the phlogiſton requiſite to form it) muſt, in the former experiment, have been retained in the iron, in order to compoſe the *water* which was now made by the union of the dephlogiſticated air imbibed by the iron and the phlogiſton contained in it: and therefore the proportion between the quantity of phlogiſton in *iron* to that which is contained in an equal weight of water, may be about 12 to 10, or more accurately to 10.4.

Had no fixed air at all been found in the reſiduum above-mentioned, it might have been concluded, that water had con-

tained the very ſame proportion of phlogiſton with iron. Since when iron that has been ſaturated with dephlogiſticated air is heated in inflammable air (in which proceſs an equal weight of water is produced, and the loſs of weight in the iron is equal to that of ſuch a quantity of dephlogiſticated air as would have been one-half of the bulk of the inflammable air which diſappears in that proceſs) it might have been concluded, that one-fifth of any quantity in water had been inflammable air.

For, neglecting the difference between the weight of dephlogiſticated and common air, which is not conſiderable, and eſtimating the latter $\frac{1}{800}$th part of water, and inflammable air at one-tenth of the weight of common air, an ounce meaſure of dephlogiſticated air will weigh .6 grain, and two ounce meaſures of inflammable air will weigh .12 grain, which numbers are to each other as 5 to 1 *.

Though, in conſequence of the ſmall quantity of fixed air which is found in the proceſs of melting iron in dephlogiſticated air, this concluſion is not accurate, it is pretty nearly ſo; and it is remarkable that, upon this ſuppoſition, about as much inflammable air is expelled from iron when water is com-

* It appears from the proſecution of theſe experiments, that the water which is found on heating the ſcales of iron in inflammable air, is not formed by the dephlogiſticated air expelled from them uniting with the inflammable air in the veſſel, but was the water previouſly contained in the ſcales, which is made to quit its place by the introduction of the phlogiſton from the inflammable air; yet that water carries out with it not much leſs phlogiſton than was taken in by the iron, and a little more muſt be allowed for that water which was neceſſary to make inflammable air, and which could not enter the iron when it was revived; ſo that, on the whole, the phlogiſton in the water that is found after the proceſs muſt be very nearly the ſame quantity that is imbibed by the iron, and the water is nearly the ſame that would have been produced, on the ſuppoſition of its being made from dephlogiſticated air expelled from the ſcales uniting with the inflammable air in the veſſel.

bined

bined with it, as the water itſelf brings along with it, as an eſſential ingredient in its compoſition. For in one experiment 296 grains added to the weight of a quantity of iron by ſteam, made it to yield about 1000 ounce meaſures of inflammable air. This would weigh 60 grains, and one-fifth of the 296 grains of water will be 59.2 grains. Again, 267 grains added to iron by ſteam made it to yield 840 ounce meaſures of inflammable air, which would weigh 50.4 grains, and one-fifth of the 267 would be 53.4 grains.

When the experiments on the melting of iron in dephlogiſticated air ſhall be repeated on a larger ſcale, which it will not be difficult to do by the help of a larger burning lens than I am at preſent poſſeſſed of, it will be eaſy to reduce theſe calculations to a greater certainty. All that I can do at preſent is to approximate to ſuch general concluſions as I have mentioned; but they are of ſo much conſequence in philoſophy, that it will certainly be well worth while to aſcertain them with as much accuracy as poſſible. Nice calculations would be ill beſtowed on the imperfect *data* which I am as yet able to furniſh. Attention muſt alſo be given to the quantity of water contained in inflammable air from iron; which not being yet aſcertained is not conſidered in theſe inferences. I wiſh only to hint in this Poſtſcript, that ſome important concluſions ſeems to be nearly within our reach.

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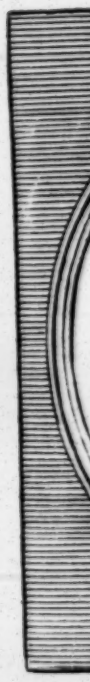

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PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

VOL. LXXV. For the Year 1785.

PART II.

Basire sc.

LONDON,

SOLD BY LOCKYER DAVIS, AND PETER ELMSLY,
PRINTERS TO THE ROYAL SOCIETY.

MDCCLXXXV.

Jos: Banks

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XVI. *Of the Rotatory Motion of a Body of any Form whatever, revolving, without Reſtraint, about any Axis paſſing through its center of Gravity.* *By Mr.* John Landen, *F.R.S.*

Read March 17, 1785.

A SPHERICAL body, uniformly denſe, it is obvious, will, if made to revolve freely about any axis paſſing through its center, continue to revolve about the ſame axis; and, by what I have ſhewn in the *Philoſophical Tranſactions* for the year 1777, it appears, that a *cylinder* of uniform denſity, whoſe length is to its radius as $\sqrt{3}$ to 1, will do the ſame. It likewiſe appears, by my *Mathematical Memoirs*,

that a *cone*, a *conoid*, a *priſm*, or a *pyramid*, &c. of certain dimenſions, will have the like property of continuing, without any reſtraint, to revolve about any axis paſſing through its center of gravity.

When the axis, about which a body may be made to revolve, is not a permanent one, the centrifugal force of its particles will diſturb its rotatory motion, ſo as to cauſe it to change its axis of rotation (and conſequently its poles) every inſtant, and endeavour to revolve about a new one: and I cannot think it will be deemed an unintereſting propoſition to determine in what track, and at what rate, the poles of ſuch momentary axis will be varied in any body whatever; as, without the knowledge to be obtained from the ſolution of ſuch problem, we cannot be certain whether the earth, or any other planet, may not, from the *inertia* of its own particles, ſo change its momentary axis, that the poles thereof ſhall approach nearer and nearer to the preſent equator, or whether the evagation of the momentary poles, ariſing from that cauſe, will not be limited by ſome known leſſer circle. Which certainly is an important conſideration in aſtronomy; eſpecially now that branch of ſcience is carried to great perfection, and the acute aſtronomer endeavours to determine the motions of the heavenly bodies with the greateſt exactneſs poſſible.

I do not know that the problem has before been ſolved by any mathematician in theſe kingdoms; but I am aware that it has been conſidered by ſome gentlemen, very eminent for their mathematical knowledge, in other nations. The ſolutions of it, given by the celebrated M. LEONHARD EULER and M. D'ALEMBERT, I have ſeen: and we learn from what the laſt mentioned gentleman has ſaid, in his *Opuſcules Mathematiques*, that

that a ſolution of it, inveſtigated by M. JOHN ALBERT EULER (after a method ſimilar to his father's) obtained the prize given by the Academy of Sciences in the year 1761. The concluſions deduced by thoſe very learned gentlemen differing greatly from mine made me ſuſpect, for ſome time, that I had ſomewhere erred in my inveſtigation, and induced me to reviſe my proceſs again and again with the greateſt circumſpection. At length my ſcrutiny has ſo removed my doubts, that, being well aſſured of the truth of my theory, I now beg leave to preſent it to the Royal Society; preſuming that it will be found not unworthy of the notice of ſuch readers, as are curious in contemplating the various motions which bodies may naturally have, in conſequence of inſtantaneous or continued impulſe.

In the *Philoſophical Tranſactions* referred to above, I gave a ſpecimen of this theory, as far as it relates to the motion of *a ſpheroid* and *a cylinder*. The improvements I have ſince made in it, enable me now to extend it to the motion of *any body whatever*, how irregular ſoever its form may be.

What I here infer therefrom will be found to differ very materially from the deductions in the ſolutions given by the gentlemen above-mentioned. They repreſent the angular velocity, and the momentum of rotation of the revolving body, as always *variable*, when the axis about which it has a tendency to revolve is a momentary one, except in a particular caſe. By my inveſtigation it appears, that the angular velocity and the momentum of rotation will always be *invariable* in any revolving body, though the axis about which it endeavours to revolve be continually varied; and the tracks of the varying poles upon the ſurface of the body are thereby determined with great facility.

It is not only obſervable, that the tracks which the varying poles take, in the ſurface of any revolving body, are ſuch that its momentum of rotation may continue the ſame whilſt its angular velocity continues the ſame; but it may be obſerved, that, in any given body, there is only one ſuch track which a momentary pole can purſue from any given point.

If the angular velocity and the momentum of rotation of a revolving body were to vary according to the computations adverted to above, it would follow, that a body might acquire an increaſe of force from its own motion, without being any way affected by any other body whatever, as the ſame percuſſive force, applied at the ſame diſtance from the momentary axis, would not always deſtroy the rotatory motion of the body, which ſurely cannot poſſibly be true. From the principles or laws of motion, which I conſider as undoubtedly true (and which indeed are no other than the common principles of mechanics), I conclude that a revolving body, not affected by any external impulſe, can no more acquire an increaſe in its momentum of rotation, than any other body, moving freely, can acquire an increaſe in its momentum, or quantity of motion, in any given direction, without being impelled by gravity or ſome other force. And the truth of this concluſion (which is hereinafter proved by other reaſoning) may be eaſily inferred from the property of the lever; ſeeing that the joint centrifugal force of the particles of the revolving body (which is the *only* diſturbing force) has no tendency to accelerate or retard their motion about the momentary axis, but only to alter the poſition of ſuch axis, the direction in which that force acts being always in a plane wherein that axis will be found.

By the theory explained in this paper, it appears that a *parallelopipedon* may always be conceived of ſuch dimenſions,

that being, by ſome force or forces, made to revolve about an axis, paſſing through its center of gravity, with a certain angular velocity, it ſhall move exactly in the ſame manner as any other given body will move, if made to revolve, by the ſame force or forces, about an axis paſſing through its center of gravity; the quantity of matter (as well as the initial angular velocity) being ſuppoſed the ſame in both bodies; and due regard being had, in the application of the moving force or forces, to the correſponding planes in the bodies. Therefore, as we may from thence always aſſign the dimenſions of a *parallelopipedon* that ſhall be affected exactly in the ſame manner as any other given body will be affected, as well with regard to the centrifugal force of the reſpective particles of the bodies, as to the action of equal percuſſive forces, or oſcillation; it will, after ſhewing how the dimenſions of ſuch *parallelopipedon* may be computed, be only neceſſary, in inveſtigating the propoſition under conſideration, to determine the tracks and velocities of the poles of the momentary axis, about which any *parallelopipedon* may be made to revolve.

Firſt then to find ſuch parallelopipedon (P), that, with reſpect to the action of ſuch forces as are mentioned above, it may be affected exactly in the ſame manner as any other given body (Q). Let it be conſidered that G (tab.X.fig.1.) being the center of gravity, N a point of ſuſpenſion, and O the correſponding center of oſcillation or percuſſion, the rectangle $GN \times GO$ will be an invariable quantity, the direction NGO continuing the ſame; and that a cylindric ſurface being deſcribed, ſuch that the center of the middle circular ſection thereof ſhall be G, and radius $= \sqrt{GN \times GO}$, and whoſe axis ſhall be perpendicular to the plane wherein the line NGO is ſuppoſed to be impelled to move; if all the matter in the body were placed

any

any where in that ſurface, ſo that G ſhould be the center of gravity of the matter ſo placed, any given force or forces, acting on the body in the plane juſt now mentioned, would cauſe the line NGO in the body to move exactly in the ſame manner as it would move, if it were carried with the matter placed in the ſaid ſurface (as before-mentioned) after having been put in motion by the action of the ſame force or forces. Moreover, let it be conſidered, that there will at leaſt be three permanent axes of rotation in the body Q, at right angles to each other (as I have proved in my *Mathematical Memoirs*); and that, ſuppoſing NGO to coincide with thoſe three axes in three ſucceſſive caſes wherein the matter in Q ſhall, in each caſe, be conceived to be placed in a cylindric ſurface as deſcribed above, we may conceive it poſſible ſo to place the matter of the body, that all of it ſhall be in each of thoſe three ſurfaces, and G ſtill continue its center of gravity. And, a computation being made accordingly, it appears, that the matter of the body Q muſt be placed, in equal quantities, at each of the eight angular points of a *parallelopipedon* (R) whoſe dimenſions (length, breadth, and thickneſs) ſhall be $\sqrt{2d^2+2f^2-2e^2}$, $\sqrt{2e^2+2f^2-2d^2}$, and $\sqrt{2d^2+2e^2-2f^2}$; d, e, and f, being the three values of $\sqrt{GN \times GO}$, when NGO is ſucceſſively a permanent axis of rotation, with reſpect to the body Q, in three directions at right angles to each other.

If Q were a parallelopipedon, it may be eaſily proved, that its dimenſions muſt be $\sqrt{6d^2+6f^2-6e^2}$, $\sqrt{6e^2+6f^2-6a^2}$, and $\sqrt{6d^2+6e^2-6f^2}$, that the correſponding parallelopipedon, at the angular points whereof the matter of Q is conceived to be placed as above, may have the ſame dimenſions as thoſe which we have found our parallelopipedon R muſt have.

Whence we may infer, that the parallelopipedon (P), which we propoſed to find, muſt have the dimenſions laſt written; namely, length, breadth, and thickneſs, reſpectively equal to $\sqrt{6d^2+6f^2-6e^2}$, $\sqrt{6e^2+6f^2-6d^2}$, and $\sqrt{6d^2+6e^2-6f^2}$: which may be confirmed by a more ſtrict demonſtration founded on the principles made uſe of in my *fourth Memoir*. For it appears by what is there proved, that the centrifugal forces of the particles of any revolving body, in two directions at right angles to each other, may be expreſſed in terms of A, B, K, and variable quantities ſhewing the poſition of the momentary axis; and that, in a parallelopipedon whoſe dimenſions (length, breadth, and thickneſs) are a, b, k; and whoſe maſs, or content, is $=M$; A will be $=\frac{Ma^2}{12}$, $B=\frac{Mb^2}{12}$, and $K=\frac{Mk^2}{12}$. If therefore a be $=\sqrt{6d^2+6f^2-6e^2}$, $b=\sqrt{6e^2+6f^2-6d^2}$, and $k=\sqrt{6d^2+6e^2-6f^2}$; in ſuch body,

$$\text{A will be} = \frac{M}{2}\times\sqrt{d^2+f^2-e^2},$$

$$\text{B} = \frac{M}{2}\times\sqrt{e^2+f^2-d^2},$$

$$\text{K} = \frac{M}{2}\times\sqrt{d^2+e^2-f^2}.$$

But, in any body whatever,

$M\times d^2$ is $=$ the ſum of all the $\sqrt{x^2+z^2}\times p$,

$M\times e^2$ $=$ the ſum of all the $\sqrt{y^2+z^2}\times p$,

$M\times f^2$ $=$ the ſum of all the $\sqrt{x^2+y^2}\times p$,

and $\frac{M}{2}\times\sqrt{d^2+e^2+f^2}=$ the ſum of all the $\sqrt{x^2+y^2+z^2}\times p$: x, y, and z correſponding to the place of the particle p in the body; x being meaſured from the center of gravity upon a permanent axis of rotation, y at right angles to x, and z at right angles to

to y in a plane to which the ſaid axis is perpendicular. Therefore,

A, which is = the ſum of all the $x^2 \times p$, will be $= \frac{M}{2} \times \overline{d^2 + f^2 - e^2}$,

B, = the ſum of all the $y^2 \times p$, $= \frac{M}{2} \times \overline{e^2 + f^2 - d^2}$,

K, = the ſum of all the $z^2 \times p$, $= \frac{M}{2} \times \overline{d^2 + e^2 - f^2}$,

Hence it is evident, that d, e, and f being determined from any body whatever, the values of A, B, and K will be the ſame in that body as in our parallelopipedon P; and that the centrifugal forces of the particles will be the ſame in both bodies. Conſequently, their motions about ſucceſſive momentary axes (whoſe poles are varied by the perturbation ariſing from thoſe forces), will be the ſame in both bodies; their initial angular velocities being the ſame; as well as the poſition of their initial momentary axes, with reſpect to the correſpondent permanent axes of rotation in each body.

Let us now proceed to find how any *parallelopipedon* will revolve about ſucceſſive momentary axes paſſing through its center of gravity: by which means, with the help of the theorem juſt now inveſtigated, we ſhall be enabled to define how any body whatever will revolve about ſuch axes; which is the chief purpoſe of this diſquiſition.

Fig. 2. and 3. The length, breadth, and thickneſs of the revolving *parallelopipedon* (P) being $2d$, $2c$, and $2b$, conceive a ſpherical ſurface without matter, whoſe center is the center of gravity of the body P, to be carried about with that body during its motion; and let the ſaid ſurface be orthographically projected, ſo that the radius upon which b is meaſured may be

repreſented

repreſented by AB; the radius upon which d is meaſured may be repreſented by AD; and the radius AC, upon which c is meaſured, may be projected into the central point A. Let P be the momentary pole, and PQ the continuation of the great circle CP. Let a denote the radius AB (=AD); g and γ the ſine and coſine of the arc CP; s and t the ſine and coſine of the arc BQ, to the ſame radius a; e the angular velocity of the body and ſpherical ſurface, meaſured at the diſtance a from the momentary axis; and M the maſs or content of the parallelopipedon ($=8bcd$).

Then the motive force E, urging the pole P towards Q, will (by what I have proved in my *Mathematical Memoirs*) be $=\frac{Me^2g\gamma}{3a^7} \times \overline{Ds^2 - Ca^2}$; and the motive force $\ddot{E}$, urging the ſame pole in a direction Po, at right angles to that in which E acts, $=\frac{Me^2g}{3a^6} \times Dst$; C and D being equal to $c^2 - b^2$ and $d^2 - b^2$ reſpectively. Let Pq be to Po as E to $\ddot{E}$; complete the parallelogram oPqr, and draw the diagonal Pr. This laſt mentioned line will (by what I have ſhewn in the *Philoſophical Tranſactions* for the year 1777) be perpendicular to the tangent to the polar track at P. Therefore P$p\ddot{p}\dddot{p}$ being the projection of that track, and Pp an indefinitely ſmall particle thereof; if pu be perpendicular to PuA, and the quantities $d^2 - c^2$, $c^2 - b^2$, be not negative; $\frac{\gamma}{a} \times \overline{Ds^2 - Ca^2}$ will be to Dst (as Pq to Po) as pu to Pu, the triangles Por and Pup being ſimilar, and or = Pq. But with reſpect to our ſpherical ſurface, pu will be to Pu as $\frac{\dot{g}s}{t}$ to $-\frac{a\dot{g}}{\gamma}$; therefore, $\overline{Ca^2 - Ds^2} \times \dot{g}$ will be $= Dgs\dot{s}$, and $\frac{\dot{g}}{g} = \frac{Ds\dot{s}}{\overline{Ca^2 - Ds^2}}$. Whence, by taking the

fluents, we have $s^2 = a^2 \times \frac{Dm^2 - C\gamma^2}{Dg^2}$, and $t^2 = a^2 \times \frac{Dn^2 - B\gamma^2}{Dg^2}$; m and n denoting the values of s and t, when g is $= a$ and $\gamma = 0$; and B being put to denote the difference $D - C = d^2 - c^2$.

If now β and δ be put to denote the cofines of BP and DP to the radius a, we ſhall, from what is done above, have

$$\beta = \frac{gt}{a} = \frac{\sqrt{Dn^2 - B\gamma^2}}{D^{\frac{1}{2}}}, \quad \delta = \frac{gs}{a} = \frac{\sqrt{Dm^2 - C\gamma^2}}{D^{\frac{1}{2}}};$$

$$\beta^2 + \gamma^2 + \delta^2 = a^2, \quad \beta\dot{\beta} + \gamma\dot{\gamma} + \delta\dot{\delta} = 0;$$

$$b^2\beta^2 + c^2\gamma^2 + d^2\delta^2 = b^2n^2 + d^2m^2, \text{ and } b^2\beta\dot{\beta} + c^2\gamma\dot{\gamma} + d^2\delta\dot{\delta} = 0.$$

Drawing AR ſo that $D^{\frac{1}{2}} \times$ ſine of BR ſhall be $= C^{\frac{1}{2}}a$, it is *very remarkable*, that the momentary pole (P) will run round about the point B, or about the point D, in the ſpherical ſurface, according as the initial pole ſhall be in the part BCR or DCR of the ſaid ſurface; that is, according as Dm^2 is leſs or greater than Ca^2: and that, if the initial pole (P) be any where in the great circle CR, the momentary pole, keeping in the arc of that circle, will continually approach nearer and nearer to the point C in the ſurface of the ſphere; but, by what follows, we ſhall find that it never can arrive at that point in any finite time!

The equation of the track of the pole in the projection to which we have hitherto referred will, it is now obvious, be $y^2 = \frac{B}{C} \times x^2 + \frac{Ca^2 - Dm^2}{B}$; x, meaſured from the center A upon AD, being $= \delta$; and y, at right angles thereto, $= \beta$.

If C be $= 0$ (that is, if c be $= b$), x will be equal to the invariable quantity m; the projected track, a *right line* parallel to AB; and the track upon the ſurface of the ſphere, a *leſſer circle* in a plane parallel to the plane of the great circle BC.

If C be = D, y will be equal to the invariable quantity n; the projected track, a *right line* parallel to AD; and the track on the ſurface of the ſphere, a *leſſer circle* in a plane parallel to the plane of the great circle CD.

If Dm^2 be $= Ca^2$ the projected track will be the *right line* AR, and $y = \left(\frac{B}{C}\right)^{\frac{1}{2}} \times x$; the track upon the ſurface of the ſphere being the *great circle* CR.

In all other caſes in this projection, the track will be an *hyperbola* whoſe center is A, ſemi-axis $Aa = \left(\frac{Ca^2 \sim Dm^2}{B}\right)^{\frac{1}{2}}$, and the other ſemi-axis $= \left(\frac{Ca^2 \sim Dm^2}{C}\right)^{\frac{1}{2}}$; the right line AR being always an *aſypmtote*.

Fig. 4. When the track is projected on a plane ACD, to which the radius AB is perpendicular (the point D being the vertex as before) the equation thereof will be $y^2 = \frac{D}{C} \times \overline{m^2 - x^2}$; x, meaſured from the center A upon AD, being $= \delta$ (as before); and y, at right angles thereto $= \gamma$. This projection of the track of the pole will therefore always be an *ellipſis* a b (or a *circle*) whoſe center is A; ſemi-axis A a $= m$; and the other ſemi-axis $= \left(\frac{D}{C}\right)^{\frac{1}{2}} \times m$: except c be $= b$; in which caſe the projected track will be a *right line* a b parallel to AC.

Fig. 5. Moreover, the equation of the track projected on the plane ABC, to which the radius AD is perpendicular, will be $y^2 = \frac{D}{B} \times \overline{n^2 - x^2}$; x, meaſured from the center A upon AB, being $= \beta$; and y, at right angles thereto, $= \gamma$. The track of the pole in this projection will therefore always be an *ellipſis*

a b (or a *circle)* whoſe center is A; ſemi-axis A a $=n$; and the other ſemi-axis $=\left|\frac{D}{B}\right|^{\frac{1}{2}} \times n$; except c be $=d$; in which caſe the projected track will be a *right line* a b parallel to AC.

With regard to the permanent axes of rotation of our parallelopipedon, it appears, by my *Mathematical Memoirs*, that if two of its dimenſions be equal (that is, when the body is a *ſquare priſm)*, any line paſſing through the center of gravity of the body, in a plane to which the other dimenſion is perpendicular, will be a permanent axis of rotation; as will the line paſſing through that center, at right angles to that plane. If all the three dimenſions be equal (that is, when the body is a *cube)*, any line whatever paſſing through the center of gravity of the body will be a permanent axis of rotation.

It is obſervable, that the momentum of rotation of the body, about the momentary axis, is found by computation always $=\frac{e}{a^2} \times \sqrt{b^2m^2+c^2a^2+a^2n^2}$, e denoting the angular velocity. But $\frac{f}{a^2} \times \sqrt{b^2m^2+c^2a^2+d^2n^2}$ is the initial momentum of rotation. Therefore, conſidering the momentum of rotation as invariable, the angular velocity will be invariable, e being always $=f$, which here denotes the initial angular velocity.

Our next buſineſs is to find the length of the track deſcribed by the momentary pole (P), upon the ſpherical ſurface; and the velocity of the pole in that track.

Fig. 2, 3. It appearing, that the motive force E is $=\frac{Me^2}{3a^5} \times \sqrt{Dm^2-Ca^2} \times \frac{\gamma}{g}$, and the motive force $\mathrm{E}'' = \frac{Me^2}{3a^4g} \times \sqrt{Dm^2-C\gamma^2} \times \sqrt{Dn^2-B\gamma^2}$; we find $\mathrm{F}=\sqrt{\mathrm{E}^2+\mathrm{E}''^2}$ (the force compounded of thoſe two forces) $=\frac{Me^2}{3a^5} \times \sqrt{D^2m^2n^2-BC\alpha^2\gamma^2}$; and, F being

to E as a to the ſine of the angle pPu, it follows, that the ſine of pPu will be $= \frac{\sqrt{Dm^2 - Ca^2} \times a\gamma}{g\sqrt{D^2m^2n^2 - BCa^2\gamma^2}}$, and its coſine $= \frac{a^2\sqrt{Dm^2 - C\gamma^2} \times \sqrt{Dn^2 - B\gamma^2}}{g\sqrt{D^2m^2n^2 - BCa^2\gamma^2}}$. Therefore, that coſine being to radius as $\left(\frac{a\dot{\gamma}}{g}\right)$ the fluxion of the arc PQ to $(\dot{z})$ the fluxion of the polar track on the ſpherical ſurface, $\dot{z}$ will be $= \frac{\dot{\gamma}\sqrt{D^2m^2n^2 - BCa^2\gamma^2}}{\sqrt{Dm^2 - C\gamma^2} \times \sqrt{Dn^2 - B\gamma^2}}$.

Now, $PpI.N$ being a quadrant of a great circle (touching the ſaid polar track at P), and NAN a diameter of that circle; if we put w to denote the diſtance of any particle (p) of the parallelopipedon from that diameter, and G to denote the accelerative force of any ſuch particle when w is $= a$; the motive force F $\left(= \frac{Me^2}{3a^5} \times \sqrt{D^2m^2n^2 - BCa^2\gamma^2}\right)$, computed above, will be $=$ $\frac{G}{a^2} \times$ *the ſum of all the* $w^2 \times p$; which ſum, by computation, is found $= \frac{M}{3} \times \frac{\sqrt{d^2 + b^2} \cdot D^2m^2n^2 - \sqrt{b^2m^2 + c^2a^2 + d^2n^2} \cdot BC\gamma^2}{D^2m^2n^2 - BCa^2\gamma^2}$. Conſequently, G will be $= \frac{e^2}{a^3} \times \frac{\left(D^2m^2n^2 - BCa^2\gamma^2\right)^{\frac{3}{2}}}{\sqrt{d^2 + b^2} \cdot D^2m^2n^2 - \sqrt{b^2m^2 + c^2a^2 + d^2n^2} \cdot BC\gamma^2}$. But, by what I have done in the *Philoſophical Tranſactions* for the year 1777, $\frac{aG}{e}$ will be $= v =$ the velocity wherewith the momentary pole changes its place in the ſpherical ſurface to which it is referred. Therefore,

v will be $= \frac{e}{a^2} \times \frac{\left(D^2m^2n^2 - BCa^2\gamma^2\right)^{\frac{3}{2}}}{\sqrt{d^2 + b^2} \cdot D^2m^2n^2 - \sqrt{b^2m^2 + c^2a^2 + d^2n^2} \cdot BC\gamma^2}$; and $\frac{\dot{z}}{v} = \dot{T}$, the fluxion of the time $= \frac{a^2\dot{\gamma}}{e} \times \frac{\sqrt{d^2 + b^2} \cdot D^2m^2n^2 - \sqrt{b^2m^2 + c^2a^2 + d^2n^2} \cdot BC\gamma^2}{\sqrt{Dm^2 - C\gamma^2} \times \sqrt{Dn^2 - B\gamma^2} \times \sqrt{D^2m^2n^2 - BCa^2\gamma^2}}$

$=$

$$= \frac{b^2m^2+c^2a^2+d^2n^2}{e} \times \left\{ \begin{array}{l} \dfrac{\dot{\gamma}}{\sqrt{Dm^2-C\gamma^2} \times \sqrt{Dn^2-B\gamma^2}} + \\ \dfrac{Dm^2-Ca^2}{b^2m^2+c^2a^2+d^2n^2} \times \dfrac{D^2m^2n^2\dot{\gamma}}{\sqrt{Dm^2-C\gamma^2} \times \sqrt{Dn^2-B\gamma^2} \times \sqrt{D^2m^2n^2-BCa^2\gamma^2}} \end{array} \right\}$$: which,

when Dm^2 is $= Ca^2$, becomes $= \dfrac{\sqrt{d^2+b^2} \cdot a^2\dot{\gamma}}{e\sqrt{BC} \cdot \sqrt{a^2-\gamma^2}}$.

It is evident, that $\dfrac{d^2+b^2}{2e\sqrt{BC}} \times a \times$ hyp. log. of $\dfrac{a+\gamma}{a-\gamma}$, the value of T in that particular caſe, will be *infinite* when γ is $= a$; and this concluſion agrees with what is ſaid above reſpecting the motion of the momentary pole along the great circle CR (fig. 2. and 3.).

I have not found, that the value of T will, in general, be aſſigned by the *arcs of the conic ſections*; but my Tables * ſhew, that it will be ſo aſſigned when Dm^2 is $= Ba^2$, and in ſome other particular caſes.

We have ſtill to inveſtigate the track of the momentary pole in the *immoveable* concave ſpherical ſurface, which we muſt conceive to ſurround our *moveable* convex ſpherical ſurface, ſuppoſing the center of both thoſe ſurfaces to coincide with the centers of gravity of our parallelopipedon: which central point is always in this diſquiſition ſuppoſed at reſt.

Let AL be the projection of part of a great circle CL, at right angles to the great circle PpLN; then will the ſine of the arc CL be $= \dfrac{\sqrt{Dm^2-Ca^2} \times \gamma}{\sqrt{D^2m^2n^2-BCa^2\gamma^2}}$; its coſine $=$

$\dfrac{D^{\frac{1}{2}}\sqrt{Da^2m^2n^2-Dm^4-Ca^2m^2+Ca^2n^2 \cdot \gamma^2}}{\sqrt{D^2m^2n^2-BCa^2\gamma^2}}$; and, the fluxion of that ſine being $= \dfrac{\sqrt{Dm^2-Ca^2} \times D^2m^2n^2\dot{\gamma}}{\left\{D^2m^2n^2-BCa^2\gamma^2\right\}^{\frac{3}{2}}}$, the fluxion of that arc (CL) will be

* *Mathematical Memoirs*, publiſhed in 1780.

=

$= \frac{\sqrt{Dm - Ca^2} \times D^{\frac{3}{2}} am^2 n^2 \dot{\gamma}}{\sqrt{D^2 m^2 n^2 - BCa^2\gamma^2} \times \sqrt{Da^2 m^2 n^2 - Dm^4 - Ca^2 m^2 + Ca^2 n^2 \cdot \gamma^2}}$. Conſequently, the ſine of the arc PL being $= \frac{a^2 \sqrt{Dm^2 - C\gamma^2} \times \sqrt{Dn^2 - B\gamma^2}}{D^{\frac{1}{2}} \sqrt{Da^2 m^2 n^2 - Dm^4 - Ca^2 m^2 + Ca^2 n^2 \cdot \gamma^2}}$; and this ſine being to radius as the fluxion of the arc CL to the meaſure of the angle of contact of the polar track on the *moveable* ſpherical ſurface with a great circle, we find that meaſure

$= \frac{\sqrt{Dm^2 - Ca^2} \times D^2 m^2 n^2 \dot{\gamma}}{\sqrt{Dm^2 - C\gamma^2} \times \sqrt{Dn^2 - B\gamma^2} \times D^2 m^2 n^2 - BCa^2\gamma^2} = \frac{\sqrt{Dm^2 - Ca^2} \times D^2 m^2 n^2 \dot{z}}{\left\{D^2 m^2 n^2 - BCa^2\gamma^2\right\}^{\frac{3}{2}}}$.

The meaſure of the angle of contact of the track of the momentary pole, in the *immoveable* ſpherical ſurface, with a great circle, will accordingly be

$e\dot{T} - \frac{\sqrt{Dm^2 - Ca^2} \times D^2 m^2 n^2 \dot{z}}{\left\{D^2 m^2 n^2 - BCa^2\gamma^2\right\}^{\frac{3}{2}}} = \frac{\sqrt{b^2 m^2 + c^2 a^2 + d^2 n^2} \cdot \dot{z}}{\sqrt{D^2 m^2 n^2 - BCa^2\gamma^2}}$: by means of which meaſure we may deſcribe, by points, the track of the momentary pole in the ſpherical ſurface laſt mentioned.

There are other methods of finding that track; but I know none that is leſs difficult than this method, or in any reſpect more ſatisfactory.

The radius of the leſſer circle, which is the circle of curvature of the polar track in our immoveable ſpherical ſurface, will be $=$

$\frac{a\dot{z}}{\sqrt{\dot{z}^2 + \text{ſq. of the meaſ. of the ang. of cont.}}} = \frac{\sqrt{D^2 m^2 n^2 - BCa^2\gamma^2}}{\sqrt{\left\{b^2 + c^2\right\}^2 \cdot m^2 + c^2 + d^2\}^2 \cdot n^2 - BC\gamma^2}}$.

When B is $= o$, or very ſmall in compariſon with D, and Dm^2 is leſs than Ca^2, the laſt mentioned radius will be equal, or nearly equal, to the invariable quantity $\frac{Dmn}{\sqrt{\left\{b^2 + d^2\right\}^2 \cdot m^2 + 4d^4 n^2}}$; the track of the pole in the immoveable ſpherical ſurface being then exactly, or very nearly, a leſſer circle. At the ſame time, the

the polar track upon the moveable ſpherical ſurface will be exactly, or very nearly, a leſſer circle whoſe radius is m.

When C is $= 0$, or very ſmall in compariſon with D, and Dm^2 is greater than Ca^2, the track of the pole in the immoveable ſpherical ſurface will be exactly, or very nearly, a leſſer circle whoſe radius is $= \frac{Dmn}{\sqrt{4c^4m^2 + \overline{c^2 + a^2}^2 \cdot n^2}}$; and then the polar track upon the moveable ſpherical ſurface will be exactly, or very nearly, a leſſer circle whoſe radius is n.

Whatever the curves may be which the momentary pole ſhall deſcribe in thoſe two ſpherical ſurfaces, the track upon the moveable ſurface will always touch and roll along the track in the immoveable ſurface (whilſt the common center of both ſurfaces remains at reſt), in the manner deſcribed in my Paper in the *Philoſophical Tranſactions* for the year 1777; the velocity of the point of contact being equal to the value of v computed above, which velocity when B is $= 0$, or very ſmall in compariſon with D, and Dm^2 is leſs than Ca^2, will be exactly, or very nearly, $= \frac{d^2 - b^2}{d^2 + b^2} \times \frac{mne}{a^2}$; and when C is $= 0$, or very ſmall in compariſon with D, and Dm^2 is greater than Ca^2, that velocity will be exactly, or very nearly, $= \frac{d^2 - c^2}{d^2 + c^2} \times \frac{mne}{a^2}$.

The polar track upon the moveable ſpherical ſurface will always roll along the convexity of the track in the immoveable ſpherical ſurface; the convexity or concavity of the former being turned towards the convexity of the latter, according as Dm^2 is greater or leſs than Ca^2. Which track in the immoveable ſpherical ſurface, when it is not circular, will touch a certain circle as often as γ, during the motion, ſhall become $= 0$; and likewiſe another parallel circle

circle as often as γ ſhall become equal to $\left|\frac{\Lambda}{C}\right|^{\frac{1}{2}} \times m$, or $\left|\frac{\Lambda}{B}\right|^{\frac{1}{2}} \times n$; the parts of the track between the points of contact being perfectly ſimilar. If Dm^2 be $= Ca^2$ (Dn^2 being then $= Ba^2$, and conſequently $\left|\frac{D}{C}\right|^{\frac{1}{2}} \times m = \left|\frac{D}{B}\right|^{\frac{1}{2}} \times n = a$), the ſaid track will make an infinite number of revolutions about a certain point, continually approaching nearer and nearer thereto, without arriving thereat in any finite time, though the length of the ſpiral ſo deſcribed cannot exceed a certain finite qnantity.

M. Euler has computed, that if the motive forces to turn the revolving body about AB, AC, AD, be reſpectively denoted by H, I, K;

$$\text{H will be} = \frac{M}{3} \cdot \frac{d^2+c^2}{a^3\Gamma} \times \text{flux. of } e\beta - \frac{M}{3a^5} \cdot B^2\gamma\delta,$$

$$\text{I} = \frac{M}{3} \cdot \frac{d^2+b^2}{a^3\dot{\Gamma}} \times \text{flux. of } e\gamma + \frac{M}{3a^5} \cdot De^2\beta\delta,$$

$$\text{K} = \frac{M}{3} \cdot \frac{c^2+b^2}{a^3\dot{\Gamma}} \times \text{flux. of } e\delta - \frac{M}{3a^5} \cdot Ce^2\beta\gamma;$$

γ being ſuppoſed to decreaſe as T increaſes: and he has put the value of each of thoſe forces (H, I, K) $= o$. In doing ſo, it ſeems to me, that he has erroneouſly aſſumed equations as generally true, which are only ſo in a particular caſe. For $\frac{M}{3a^5} \cdot Be^2\gamma\delta$ is the motive force to turn the body about AB, ariſing from the centrifugal force of its particles revolving about the momentary axis AP, ſuppoſing the pole to keep its place; and $\frac{M}{3} \cdot \frac{d^2+c^2}{a^3\dot{\Gamma}} \times$ flux. of $e\beta$ is the value of the motive force requiſite to cauſe the *whole* variation of the velocity $\left(\frac{e\beta}{a}\right)$ about AB. But the firſt mentioned force *alone* does not, in general,

cauſe *all* the variation of the velocity about AB; that velocity varies in conſequence of the evagation of the pole P; and that evagation is cauſed by the motive forces urging the body to turn about AB, AC, AD, *conjunctly*. Therefore the motive force $\frac{M}{3a^5} \cdot Be^2\gamma\delta$ about AB *only* will not, in general, be equal to $\frac{M}{3} \cdot \frac{d^2+c^2}{a^3\dot{T}} \times$ flux. of $e\beta$, the value of the *whole* motive force requiſite to cauſe the variation of the velocity $\frac{e\beta}{a}$, as M. EULER reckoned.

The like objection may, I conceive, be juſtly made to his other two equations ſimilar to that which is here particularly adverted to.

M. D'ALEMBERT's radical errors, in treating this ſubject, appear to me nearly ſimilar to M. EULER's.

Other arguments may be adduced to prove, that the equations aſſumed by thoſe gentlemen are not well founded. If the forces to turn the body about the lines AB, AC, AD were each = o, the velocities about thoſe lines muſt each remain invariable; but it ſeems abſolutely impoſſible that they can ever remain ſo, whilſt the angles which thoſe lines make with the momentary axis are each continually varying. Moreover, according to their concluſions, the tangent at P to the track of polar evagation, upon the moveable ſpherical ſurface, will not always be perpendicular to the direction in which the pole P will be urged to turn by the joint centrifugal force of the particles of the revolving body; whereas it is proved, I preſume, beyond a doubt, in my Paper above-mentioned, that the ſaid track will always be interſected at right angles by the direction in which the momentary pole ſhall, at any inſtant of time, be urged to turn by the force cauſing its evagation.

If we reſolve each of the three forces H, I, K, into two others; the one to turn the body about the diameter NAN, and the other to turn it about the momentary axis PAP, at right angles to that diameter; the forces to turn it in the laſt mentioned direction, ariſing from the ſaid forces H, I, K, will be

$$\frac{\beta H}{a} = \frac{M}{3} \cdot \frac{d^2+c^2}{a^4 \dot{T}} \times \beta \text{ flux. of } e\beta - \frac{M}{3a^6} \cdot B e^2 \beta\gamma\delta,$$

$$\frac{\gamma I}{a} = \frac{M}{3} \cdot \frac{d^2+b^2}{a^4 \dot{T}} \times \gamma \text{ flux. of } e\gamma + \frac{M}{3a^6} \cdot D e^2 \beta\gamma\delta,$$

$$\frac{\delta K}{a} = \frac{M}{3} \cdot \frac{c^2+b^2}{a^4 \dot{T}} \times \delta \text{ flux. of } e\delta - \frac{M}{3a^6} \cdot C e^2 \beta\gamma\delta.$$

The ſum of theſe forces, it is obvious, muſt be $=0$; the direction wherein they are ſuppoſed to act being at right angles to that in which the body will be actually urged to turn by the joint centrifugal force of its particles, and that being the only force whereby the motion of the body is ſuppoſed to be affected: which ſum ($B+C-D$ being $=0$) is, when divided by $\frac{M}{3a^4\dot{T}}$,

$$= \left\{ \begin{array}{l} \overline{d^2+c^2} \cdot \beta^2 \dot{e} + \overline{d^2+b^2} \cdot \gamma^2 \dot{e} + \overline{c^2+b^2} \cdot \delta^2 \dot{e} \\ \overline{d^2+c^2} \cdot e\beta\dot{\beta} + \overline{d^2+b^2} \cdot e\gamma\dot{\gamma} + \overline{c^2+b^2} \cdot e\delta\dot{\delta} \end{array} \right\} = 0.$$

But $\beta\dot{\beta}+\gamma\dot{\gamma}+\delta\dot{\delta}$ being before found $=0$, we have $\overline{d^2+c^2+b^2} \times \overline{\beta\dot{\beta}+\gamma\dot{\gamma}+\delta\dot{\delta}} = 0$; and $b^2\beta\dot{\beta}+c^2\gamma\dot{\gamma}+d^2\delta\dot{\delta}$ being alſo found $=0$; it evidently follows, that

$\overline{d^2+c^2} \cdot \beta\dot{\beta} + \overline{d^2+b^2} \cdot \gamma\dot{\gamma} + \overline{c^2+b^2} \cdot \delta\dot{\delta}$ will be $=0$.

Therefore $\overline{d^2+c^2} \cdot \beta^2\dot{e} + \overline{d^2+b^2} \cdot \gamma^2\dot{e} + \overline{c^2+b^2} \cdot \delta^2\dot{e}$ will be $=0$: conſequently $\dot{e}$ will be $=0$, and e invariable; which agrees with what is ſaid above reſpecting the momentum of rotation.

The other forces ariſing by reſolution from the forces H, I, K, to turn the body about the diameter NAN, will be

$$-\frac{B\gamma\delta H}{S}=-\frac{M}{3a^3}\cdot\frac{d^2+c^2}{S\dot{T}}\cdot Be\gamma\delta\dot{\beta}+\frac{Me^2}{3a^5S}\cdot B^2\gamma^2\delta^2,$$

$$\frac{D\beta\delta I}{S}=\frac{M}{3a^3}\cdot\frac{d^2+b^2}{S\dot{T}}\cdot De\beta\delta\dot{\gamma}+\frac{Me^2}{3a^5S}\cdot D^2\beta^2\delta^2,$$

$$-\frac{C\beta\gamma K}{S}=-\frac{M}{3a^3}\cdot\frac{c^2+b^2}{S\dot{T}}\cdot Ce\beta\gamma\dot{\delta}+\frac{M}{3a^5S}\cdot C^2\beta^2\gamma^2;$$

$$S\text{ being}=\sqrt{D^2m^2n^2-BCa^2\gamma^2}.$$

And, no external force being ſuppoſed to act on the body, it follows, that the ſum of theſe three forces muſt be $=0$: therefore we may infer, that

$\dot{T}$ will be $=\frac{a^2}{e}\times\frac{\sqrt{a^4-c^4}\cdot\gamma\delta\dot{\beta}-\sqrt{d^4-b^4}\cdot\beta\delta\dot{\gamma}+\sqrt{c^4-b^4}\cdot\beta\gamma\dot{\delta}}{B^2\gamma^2\delta^2+D^2\beta^2\delta^2+C^2\beta^2\gamma^2}$; which agreeing with the value of $\dot{T}$ found above, the truth of our preceding proceſs is thus confirmed.

The force $\frac{Me^2}{3a^5S}\times\sqrt{B^2\gamma^2\delta^2+D^2\beta^2\delta^2+C^2\beta^2\gamma^2}$, ariſing from thoſe three forces, is the *whole* joint centrifugal force of the particles of the revolving body, to turn it about the diameter NAN the way it will actually be urged to turn by ſuch force; the value whereof ſo computed will be $\left(=\frac{Me^2}{3a^5}\times\sqrt{D^2m^2n^2-BCa^2\gamma^2}=\frac{Me^2S}{3a^5}\right)$ equal to the value of the force F computed above, both being conſidered as urging the body to turn in the ſame direction. And the quantity

$$\frac{Me}{3a^3S\dot{T}}\times\sqrt{d^2+c^2}\cdot\gamma\delta\dot{\beta}-\sqrt{d^2+b^2}\cdot D\beta\delta\dot{\gamma}+\sqrt{c^2+b^2}\cdot C\beta\gamma\dot{\delta}$$

$\left(=\frac{e\dot{z}}{a^3\dot{T}}\times\textit{the ſum of all the }w^2\times p\right)$ is the value of the motive force which, acting in that very direction, is requiſite to cauſe the momentary pole to change its place as above deſcribed. Thus we ſee diſtinctly how the equation ariſes, by which the value of $\dot{T}$ is juſt now determined. I do

I do not find that the reſolving the forces H, I, K, in any other manner will conduce to the attainment of any uſeful concluſion.

It appears, by what is done above, that the force

$$H \text{ is} = \frac{Me}{3a^3R\dot{T}} \times CD^2m^2\beta^2\dot{\beta},$$

$$I \text{ is} = \frac{Me}{3a^3R\dot{T}} \times BC \,.\, \overline{Cn^2 - Bm^2} \,.\, \gamma^2\dot{\gamma},$$

$$K = \frac{-Me}{3a^3R\dot{T}} \times BD^2n^2\delta^2\dot{\delta};$$

$$R \text{ being} = B^2\gamma^2\delta^2 + D^2\beta^2\delta^2 + C^2\beta^2\gamma^2.$$

And it is obvious, that each of the three laſt mentioned forces will be $=0$, if any two of the quantities b, c, d, be equal; two of the values of thoſe forces then vaniſhing, by reaſon of that equality; and the third value alſo vaniſhing by either $\dot{\beta}$, $\dot{\gamma}$, or $\dot{\delta}$, being at the ſame time $=0$. Therefore, in that caſe it happens, that M. EULER's computation agrees with mine: in every other caſe, I am clearly of opinion, his concluſions are not true. The ſame may be ſaid of M. D'ALEMBERT's concluſions reſpecting the ſame propoſition.

The evagation of the pole of a revolving body conſidered above, does not ariſe from gravity, the attraction of any other body, or any external impulſe whatever; but is only the conſequence of the *inertia of matter*, and muſt neceſſarily enſue, according to the theory here explained, in every body in the univerſe, after having been made to revolve, without reſtraint, about any line paſſing through its center of gravity, that is not *a permanent* axis of rotation.

The Earth being neither uniformly denſe nor a perfect ſpheroid muſt, in ſtrictneſs, be conſidered as having only three permanent axes of rotation, agreeably to what I have proved in my *Mathematical Memoirs*; and, as it is diſturbed in its rotatory motion by the attraction of the ſun and moon (and

other

other bodies in our ſyſtem); it follows; that it will not continually revolve about either of thoſe axes, but will revolve, or endeavour to revolve, about ſucceſſive momentary axes, as ſhewn above. If then its three permanent axes of rotation be called its *firſt*, *ſecond*, and *third* axes; and the poles of its *firſt* axis be thoſe about which its momentary poles are carried according to our theory; the *ſecond* and *third* axes will be in the plane of its equator, the three being at right angles to each other. Therefore, with reſpect to the above theory, this terreſtrial maſs muſt be conſidered of ſuch a form, that its equator, and any ſection parallel thereto, ſhall rather be elliptical than circular. And, denoting its firſt, ſecond, and third axes by b, c, d, reſpectively, obſervations evince, that the difference $c-b$ will be much greater than the difference $d-c$. Whence it follows, that (ſuppoſing the earth's rotatory motion to be diſturbed *only* by the centrifugal force ariſing from the *inertia* of its own particles) the track of polar evagation with us will be nearly circular, and the radius of the limiting circle very ſmall, whether we have regard to the moveable or immoveable ſpherical ſurface referred to above; but that, in the latter ſurface, ſuch circle will be much leſs than in the former: and it moreover follows, that the concavity of the track upon the moveable ſurface will continually touch and roll along the convexity of the track in the immoveable ſurface.

In other planets, the tracks of polar evagation may, from a ſimilar cauſe, be very different. The theory above explained evidently proves, that their axes of rotation may poſſibly vary greatly in poſition, merely through the *inertia of matter*; whilſt Providence has ſo ordered it, that the poſition of the axes of rotation of this planet ſhall, by that cauſe, be but very little altered.

Fig. 3.

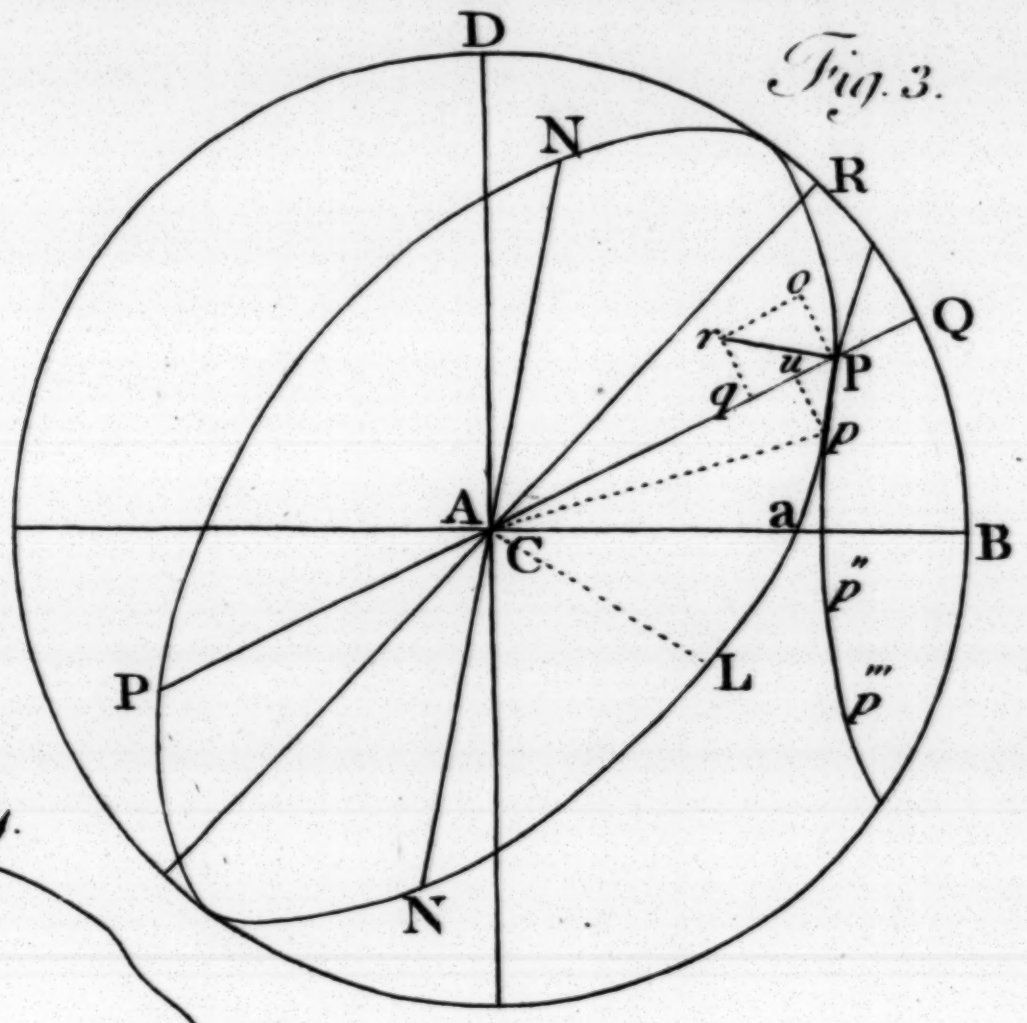


Fig. 4.

Fig. 5.

Fig. 2.

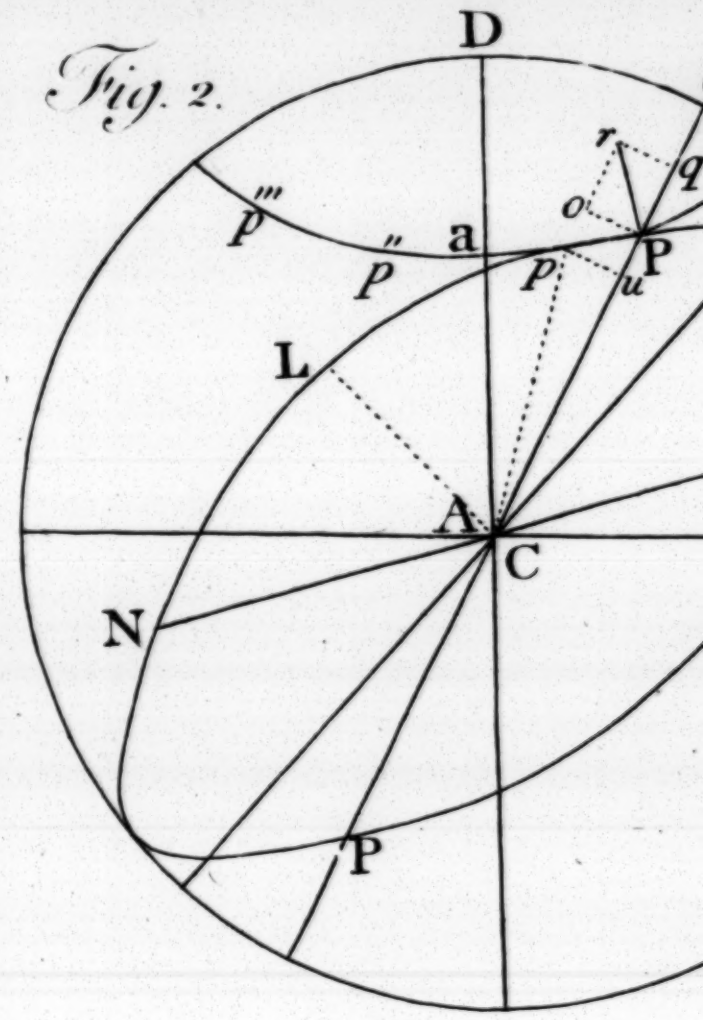
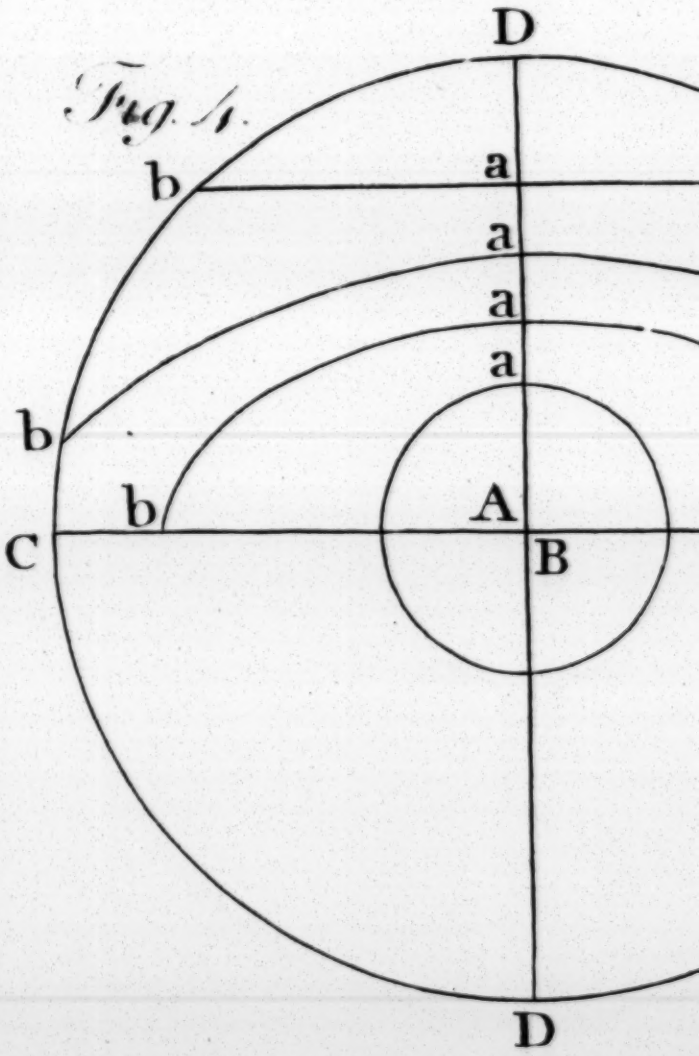


Fig. 4.



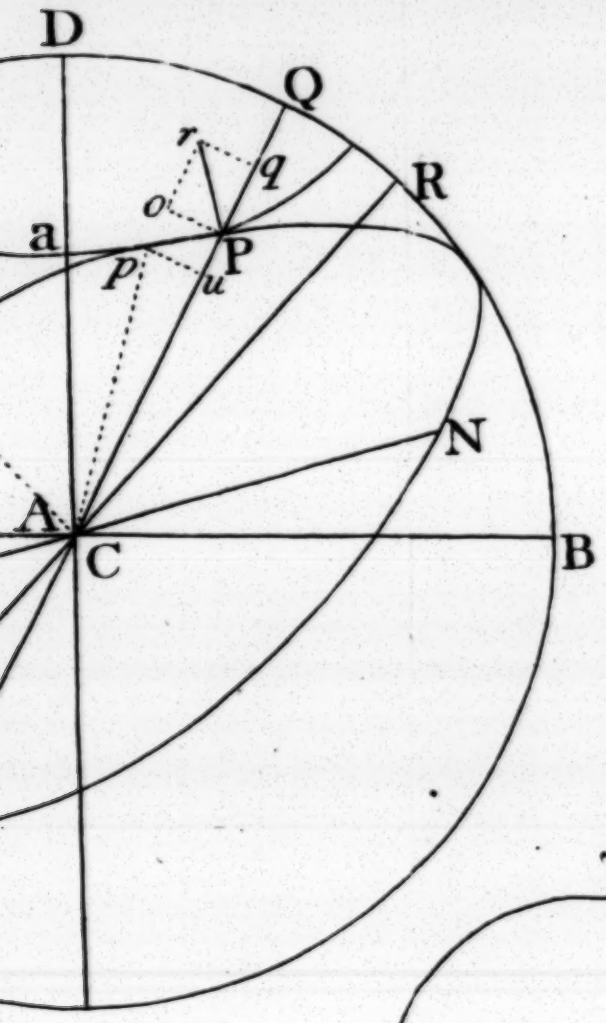
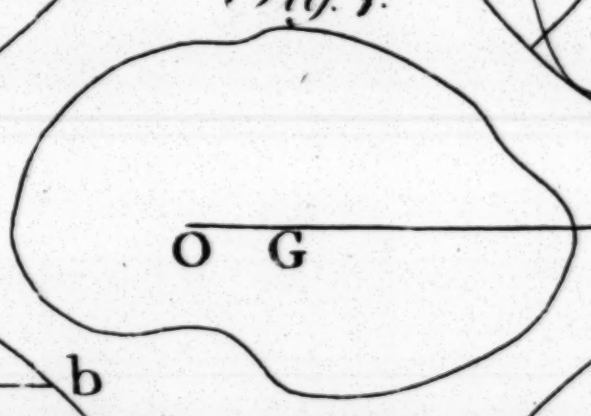


Fig. 4.



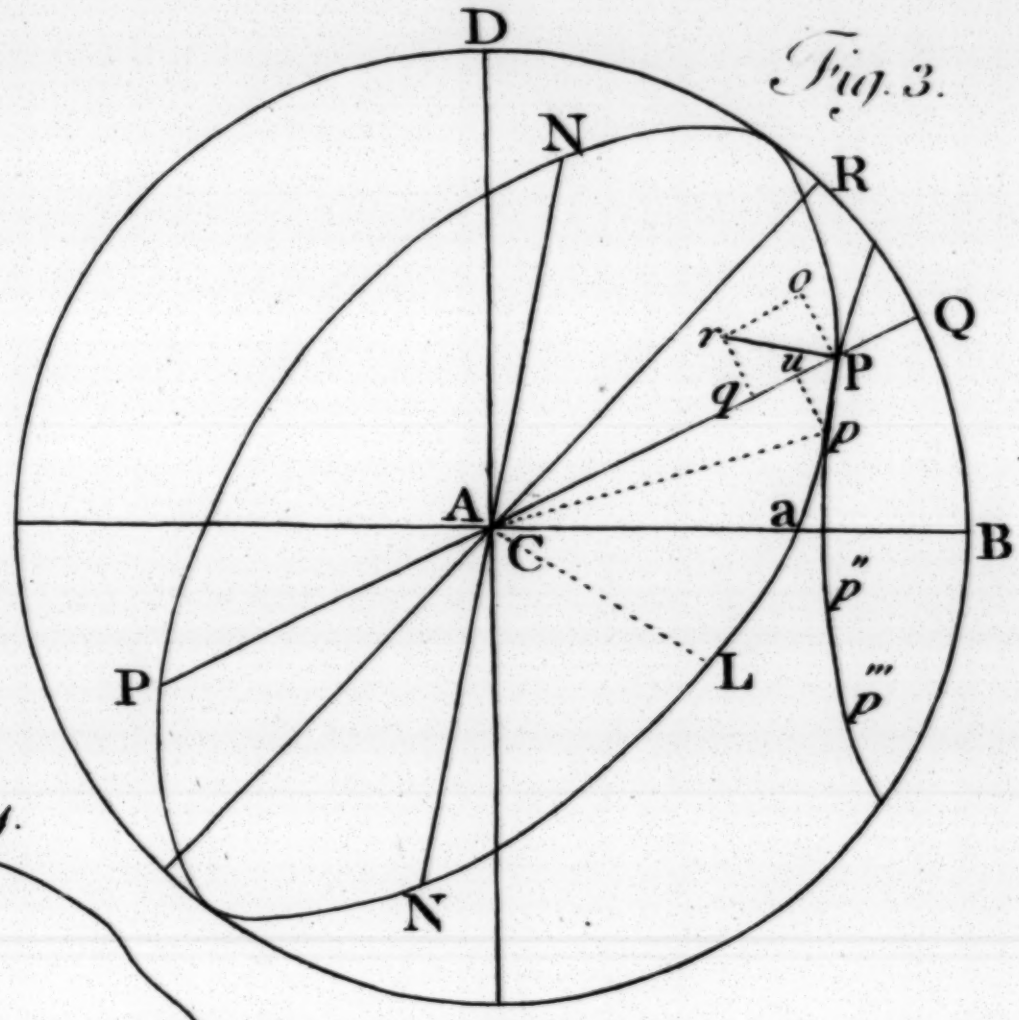
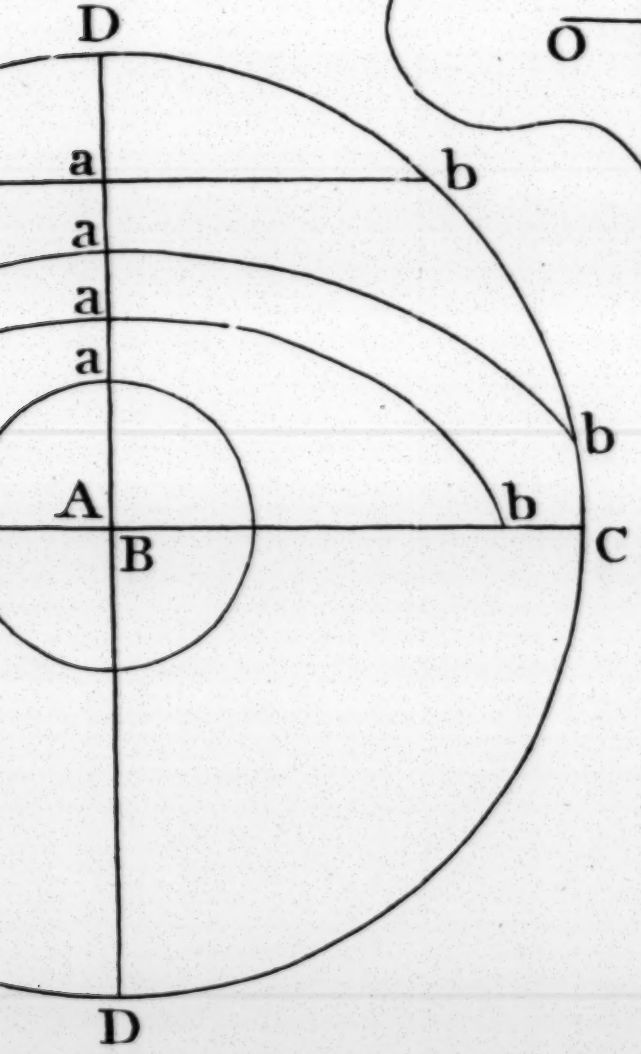


Fig. 3.



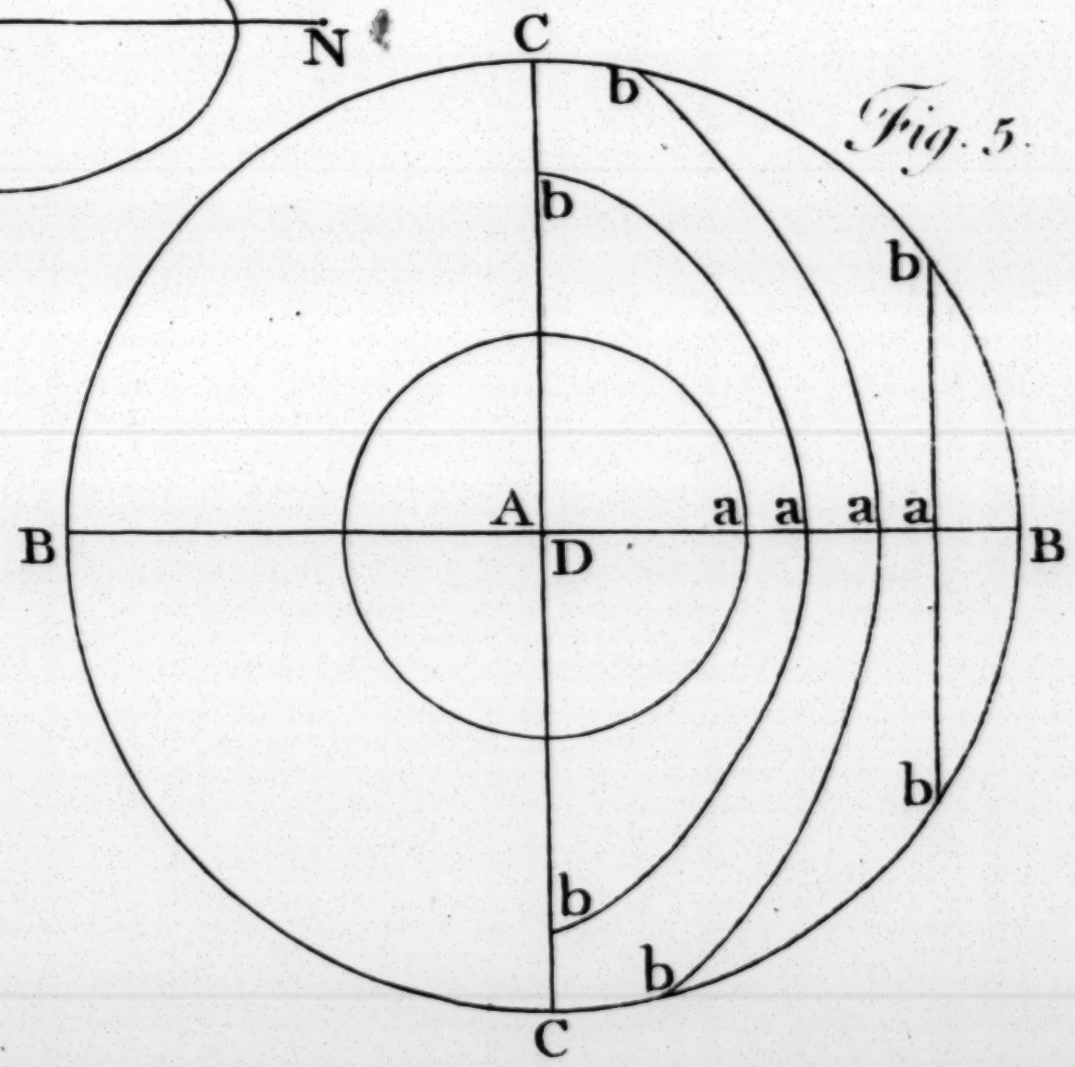


Fig. 5.

XVII. *Deſcription of a new Marine Animal. In a Letter from Mr.* Everard Home, *Surgeon, to* John Hunter, *Eſq. F.R.S. With a Poſtſcript by Mr.* Hunter, *containing anatomical Remarks upon the ſame.*

Read March 7, 1785.

TO JOHN HUNTER, ESQ. F.R.S.

DEAR SIR, Sept. 20, 1784.

I SENT you, about three years ago, a ſea animal from Barbadoes, which was unlike any one I had ever ſeen. From the want of books and other information in that iſland, I was unable at the time to find out, whether it was a new acquiſition, or had been deſcribed by any authors in natural hiſtory.

Since my arrival in England, I have examined the libraries of ſome men of ſcience for an account of this animal, and have made other enquiries among the naturaliſts, without ſucceſs. The ſpecimen I ſent you was found on a part of the coaſt which had undergone very remarkable changes, in conſequence of a violent hurricane. Theſe changes were indeed the means of its being diſcovered, and preſent a probable reaſon why it was not diſcovered before. The extraordinary circumſtances which brought it within our reach, and the ſilence of all the authors on natural hiſtory which I have been able to conſult, incline me to believe it to be a non-deſcript. As the peculiarities of its ſtructure may add to the knowledge of the natural

hiſtory

hiſtory of other animals of this genus, at preſent ſo little underſtood, I have drawn out a more particular account of it; which, if you think it deſerves attention, you may preſent to the Royal Society.

This animal was found on the ſouth-eaſt coaſt of Barbadoes, cloſe to Charles Fort, about a mile from Bridge Town, in ſome ſhoal water, ſeparated from the ſea by the ſtones and ſand thrown up by the dreadful hurricane, which happened in the year 1780, and did ſo much miſchief to the iſland.

The wind, in the beginning of the ſtorm, which was in the afternoon, blew very furiouſly from the north-weſt, making a prodigious ſwell in the ſea; and in the middle of the night changing ſuddenly to the ſouth-eaſt, it blew from that quarter upon the ſea, already agitated, forcing it upon the ſhore with ſo much violence, that it threw down the rampart of Fort Charles, which was oppoſed to it, although thirty feet broad, by the burſting of one ſea. It forced up, at the ſame time, immenſe quantities of large coral rocks from the bottom of the bay, making a reef along this part of the coaſt for the extent of ſeveral miles, at only a few yards diſtance from the ſhore.

The ſoundings of the harbour were found afterwards to be intirely changed, by the quantity of materials removed from the bottom in different places. In the reef of coral was found an infinite number of large pieces of brain-ſtone, containing the ſhell of this animal; but the animals had either been long dead, or more probably deſtroyed by the motion of the rocks in the ſtorm: ſome few of the brain-ſtones, however, that had been thrown beyond the reef, and lodged in the ſhoal water, receiving leſs injury, the animals were preſerved unhurt.

The animal, with the ſhell, is almoſt intirely incloſed in the brain-ſtone, ſo that at the depth in which they generally lie,

they are hardly diſcernible, through the water, from the common ſurface of the brain-ſtone; but when in ſearch of food they throw out two cones, with membranes twiſted round them in a ſpiral manner, which have a looſe fringed edge, looking at the bottom of the ſea like two flowers; and in this ſtate they were diſcovered.

The ſpecies of Actinia called in Barbadoes the Animal Flower, and common to many parts of that iſland, although rarely before ſeen on this part of the coaſt, was now found in conſiderable numbers in this ſhoal water.

The animal was firſt obſerved by Captain HENDIE, the officer commanding Fort Charles, in looking for ſhells which were thrown up in great numbers from the bottom of the harbour. He found a piece of brain-ſtone containing three of them in different parts of it. Some little time after, I was lucky enough to find another brain-ſtone with two in it; one of them is the ſpecimen in your poſſeſſion; the other was deſtined for examination, of which the following is the account.

The animal, when taken out of the ſhell, including the two cones and their membranes, is five inches in length; of which the body is three inches and three-quarters, and the apparatus for catching its prey, which may be conſidered as its tentacula, about an inch and a quarter.

The body of the animal is attached to its ſhell, for about three-quarters of an inch in length, at the anterior part where the two cones ariſe, by means of two cartilaginous ſubſtances, with one ſide adapted to the body of the animal, the other to the internal ſurface of the ſhell: the reſt of the body is unattached, of a darkiſh white colour, about half an inch broad, a little flattened, and rather narrower towards the tail. The muſcular fibres upon its back are tranſverſe; thoſe on the belly

longitudinal, making a band the whole length of the body, on the edge of which the tranſverſe fibres running acroſs the back terminate.

The two cartilaginous ſubſtances by which the animal adheres to its ſhell, are placed one on each ſide of the body, and are joined together upon the back of the animal at their poſterior edges: they are about three-quarters of an inch long, are very narrow at their anterior end, becoming broader as they go backwards; and at their poſterior end they are the whole breadth of the body of the animal. Upon their external ſurface there are ſix tranſverſe ridges, or narrow folds; and along their external edges, at the end or termination of each ridge, is a little eminence reſembling the point of a hair pencil, ſo that on each ſide of the animal there are ſix of theſe little projecting ſtuds, for the purpoſe of adhering to the ſides of the ſhell in which the animal is incloſed. The internal ſurfaces of theſe cartilages are firmly attached to the body of the animal, in their middle part, by a kind of band or ligament; but the upper and lower ends are lying looſe.

From the end of the body, between the two upper ends of theſe cartilages, ariſe what I ſuppoſe to be the tentacula, conſiſting of two cones, each having a ſpiral membrane twining round it: they are cloſe to each other at their baſes, and diverge as they riſe up, being about an inch and a quarter in length, and nearly one-ſixth of an inch in thickneſs at their baſe, and gradually diminiſhing till they terminate in points. The membranes which twine round theſe cones alſo take their origin from the body of the animal, and make five ſpiral turns and a half round each, being loſt in the points of the cones; they are looſe from the cone at the loweſt ſpiral turn which they make, and are nearly half an inch in breadth; they are exceed-

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ingly delicate, and have at ſmall diſtances fibres running acroſs them from their attachment at the ſtem to the looſe edge, which gives them a ribbed appearance. Theſe fibres are continued about one-tenth of an inch beyond the membrane, having their edges finely ſerrated, like the tentacula of the Actiniæ found in Barbadoes: theſe tentacula ſhorten as the ſpiral turns become ſmaller, and are entirely loſt in that part of the membrane which terminates in the point of the cone.

Behind the origin of theſe cones ariſes a ſmall ſhell, which, for one-ſixth of an inch from its attachment to the animal, is very ſlender: it is about three quarters of an inch in length, becoming conſiderably broader at the other end, which is flat, and about one-third of an inch broad; the flattened extremity is covered with a kind of hair, and has riſing out of it two ſmall claws, about one-ſixth of an inch in length. If the hair, and mucus entangled in it, be taken away, this extremity of the ſhell becomes concave, is of a pink colour, and the two claws riſing out from its middle part have each three ſhort branches, not unlike the horns of a deer. The body of this ſhell has a ſoft cartilaginous covering, with an irregular but poliſhed ſurface: on this the cones reſt in their collapſed ſtate, in which ſtate the whole of the ſhell is drawn into the cavity of the brain-ſtone, excepting the flattened end with the two claws.

Before the cones there is a thin membrane, which appears to be of the ſame length with the ſhell juſt deſcribed. In the collapſed ſtate it lies between the cones and the ſhell in which the animal is incloſed; but, when the tentacula are thrown out, it is alſo protruded.

The ſhell of this animal is a tube, which is very thin, and adapted to its body: the internal ſurface is ſmooth, and of a

pinkiſh white colour: its outer ſurface is covered by the brain-ſtone in which it is incloſed, and the turnings and windings which it makes are very numerous. The end of the ſhell, which opens externally, riſes above the ſurface of the ſtone on one ſide half an inch in height, for about half the circumference of the aperture, bending a little forwards over it, and becoming narrower and narrower as it goes up, terminating at laſt in a point juſt over the center of the opening of the ſhell; on the other ſide it forms a round margin to the ſurface of the brain-ſtone. This part of the ſhell is much thicker and ſtronger than that part which is incloſed in the brain-ſtone: its outer ſurface is of a darkiſh brown colour; its inner of a pinkiſh white.

The animal, when at reſt, is wholly concealed in its ſhell; but when it ſeeks for food, the moveable ſhell is puſhed ſlowly out with the cones and their membranes in a collapſed ſtate; and when the whole is expoſed, the moveable ſhell falls a little back, and the membrane round each of the cones is expanded, the tentacula at the baſes of the cones having juſt room enough to move without touching one another. The thin membrane which lay between the cones and the incloſing ſhell is protruded in the form of a fold, and lies over the external ſhell which projects from the brain-ſtone.

The membranes have a ſlow ſpiral motion, which continues during the whole time of their being expanded; and the tentacula upon their edges are in conſtant action. The motion of the membrane of the one cone ſeems to be a little different from that of the other, and they change from the one kind of motion to the other alternately, a variation in the colour of the membrane at the ſame time taking place, either becoming a ſhade lighter or darker; and this change in the colour, while

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the whole is in motion, produces a pleaſing effect, and is moſt ſtriking when the ſun is very bright. The membranes, however, at ſome particular times appear to be of the ſame colour.

While the membranes are in motion, a little mucus is often ſeparated from the tentacula at the point of the cone. Upon the leaſt motion being given to the water, the cones are immediately, and very ſuddenly, drawn in.

This apparatus for catching food is the moſt delicate and complicated that I have ſeen; but I ſhall not trouble you with any conjectures upon what that food may be, as I have not attained ſufficient knowledge of the animal to ſpeak with the ſmalleſt certainty.

I have endeavoured to deſcribe the external appearances as I ſaw them; and have annexed two drawings of the animal in its two different ſtates, one in ſearch of food, and one while lying at reſt; theſe are a little magnified, to ſhow the parts more diſtinctly.

I ſhall not ſay any thing of the internal parts, or their uſes, as the animal is in your poſſeſſion, who are ſo much better able to explain its internal œconomy.

I am, &c.

EVERARD HOME.

POST-

POSTSCRIPT,

BY JOHN HUNTER, ESQ. F. R. S.

ANIMALS which come from foreign countries, and cannot be brought to England alive, muſt be kept in ſpirits to preſerve them from putrefaction, which makes them leſs fitted for anatomical examination; for the ſpirits, which preſerve them, produce a change in many of their properties, and alter the natural colours, and texture of the parts, ſo that often the ſtructure alone of the animal can be aſcertained; and where this is not naturally diſtinct, it becomes frequently intirely obſcured, and the texture of the finer parts is wholly deſtroyed, requiring a very extenſive knowledge of ſuch parts in animals at large, to aſſiſt us in bringing them to light: this happens to be the caſe with the animal whoſe diſſection is the ſubject of this Poſtſcript.

The animal may be ſaid to conſiſt of a fleſhy covering, a ſtomach and inteſtinal canal, and the two cones with their tentacula and moveable ſhell, which laſt may be conſidered as appendages.

The body of the animal is flattened, and terminates in two edges, which are interſected by rugæ, the faſciculi of tranſverſe muſcular fibres which run acroſs the back being continued over them. Upon each of theſe edges is placed a row of fine hairs, which project to ſome diſtance from the ſkin.

The fleſhy covering conſiſts principally of muſcular fibres: thoſe upon the back are placed tranſverſely, to contract the body

laterally; thoſe on the belly longitudinally, to ſhorten the animal when ſtretched out, and to draw it into the ſhell.

The ſtomach and inteſtine make one ſtraight canal: the anterior end of this forms the mouth, which opens into the grooves made by the ſpiral turns of the tentacula round the ſtem of each of the cones; and the inteſtine at the poſterior end opens externally, forming the anus. From the contracted ſtate of the animal, the inteſtine is thrown into a number of folds.

On examining the cones and the tentacula, I at firſt believed that the ſpiral form aroſe from their being in a contracted ſtate; and that, when the tentacula were erected, the cone untwiſted, forming a longer cone with the tentacula ariſing from its ſides, like the plume from the ſtem of a feather; and that this ſtem was drawn in or ſhortened by means of a muſcle paſſing along the center, which threw the tentacula into a ſpiral line, ſimilar to the penis's of many birds; but how far this is really the caſe, I have not been able to aſcertain.

The internal ſtructure of this animal, like moſt of thoſe which have tentacula, is very ſimple; it differs, however, materially from many, in having an anus, moſt animals of this tribe, as the Polypi, having only one opening, by which the food is received, and the excrementitious part of it alſo afterwards thrown out; this we muſt have ſuppoſed, from analogy, to take place in the animal which is here deſcribed, more particularly ſince it is incloſed in a hard ſhell, at the bottom of which there appears to be no outlet; but as there is an anus this cannot be the caſe.

It is very ſingular, that in the Leach, Polypi, &c. where no apparent inconvenience can ariſe from having an anus, there is not one, while in this animal, where it would ſeem to be attended with many, we find one; but there being no anus

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in the Leach, Polypi, &c. may depend upon ſome circumſtance in the animal œconomy which we are at preſent not fully acquainted with.

The univalves, whoſe bodies are under ſimilar circumſtances reſpecting the ſhell with this animal, have the inteſtine reflected back, and the anus, by that means, brought near to the external opening of the ſhell, the more readily to diſcharge the excrement; and although this ſtructure, in theſe animals, appears to be ſolely intended to anſwer that purpoſe, yet when we find the ſame ſtructure in the black Snail, which has no ſhell, this reaſoning will not wholly apply, and we muſt refer it to ſome other intention in the animal œconomy.

In this animal we muſt therefore reſt ſatisfied that the diſadvantageous ſituation of the anus, with reſpect to the excrement's being diſcharged from the ſhell, anſwers ſome purpoſe in the œconomy of the animal, which more than counter-balances the inconveniences produced by it.

It would appear, from conſidering all the circumſtances, that the excrement thrown out at the anus muſt paſs from the tail along the inſide of the tube, between it and the body of the animal, till it comes to the external opening of the ſhell, as there is no other evident mode of diſcharging it.

How the tube or ſhell is formed in ſtone or coral is not eaſily aſcertained. It may be aſked, whether this animal has the power of boring backwards as the Teredo Navalis probably does, or whether the ſtone or coral is formed at the ſame time with the animal, and grows and increaſes with it: and if we conſider all the circumſtances, this laſt would appear to be moſt probable, and agree beſt with the different phænomena; for the coral is lined with a ſhell, which could not be the caſe if the animal was continually increaſing this hole, both in length and

breadth, in proportion to its growth; but if the coral and the animal increaſe together, it is then ſimilar to the growth of all ſhells, whether bivalve or univalve.

The animal does not appear to have the power of increaſing its canal, being only compoſed of ſoft parts. This, however, is no argument againſt its doing it, for every ſhell fiſh has the power of removing a part of its ſhell, ſo as to adapt the new and the old together; which is not done by any mechanical power, but by abſorption.

The tribe of animals which have tentacula conſiſts of an almoſt infinite variety, and many of the ſpecies have been deſcribed. Of that kind, however, which has the double cones, I believe hitherto no account has been given. It is moſt probably to be found in the ſeas ſurrounding the different iſlands in the Weſt Indies; for I received an animal, ſome years ago, from Mr. OLIVER, ſurgeon, at Tenby in Pembrokeſhire, which he had procured from a gentleman at St. Vincent's; which, upon examination, proves to be the ſame animal with that above deſcribed, only that the moveable ſhell is wanting.

Since I began this Poſtſcript, I find there is a deſcription of a double-coned Terebella, publiſhed by the rev. Mr. CORDINER, at Bamf in Scotland, which was found upon that coaſt; in which the cones have their tentacula paſſing out from the end, and when erected they ſpread from the cone as from a center. This proves that the double-coned tentacula alſo have different ſpecies.

EXPLANATION OF THE FIGURES, TAB. XI.

FIG. I.

A drawing of the animal after death, as it appeared in ſpirits, a little magnified.

A. The under ſide of the body.

BB. The cartilages which attach the animal to the ſides of the cavity in which it lies.

C. One of the cones covered by its membrane in a collapſed ſtate.

D. The loweſt ſpiral turn of the membrane and its tentacula ſpread out.

EE. The cut edges of the divided membrane, which are turned on each ſide to ſhew the cone.

F. The cone as it appears in the intervals between the ſpiral turns of the membrane.

G. The moveable ſhell, with the ſmooth cartilaginous covering, in an outſide view.

H. The flattened end of the moveable ſhell, with hair upon it.

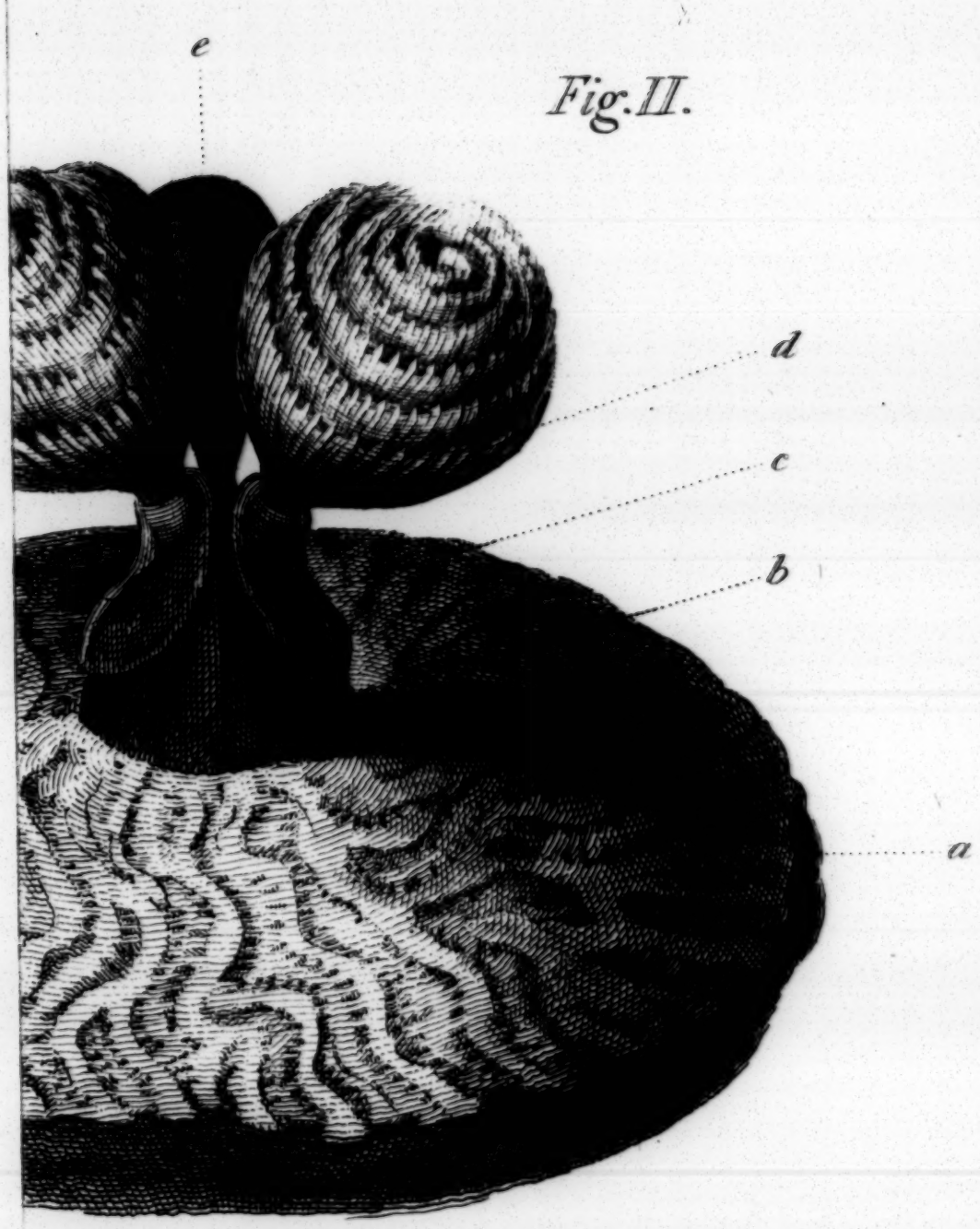
II. The two claws that ariſe from the ſurface of the flattened end of the moveable ſhell.

K. The anus, into which a hog's briſtle is introduced.

FIG. II.

A drawing of the animal, with its tentacula expanded in ſearch of food, as it appears in the ſea; taken from a ſketch made in Barbadoes, where no draughtſman could be procured

Fig. II.



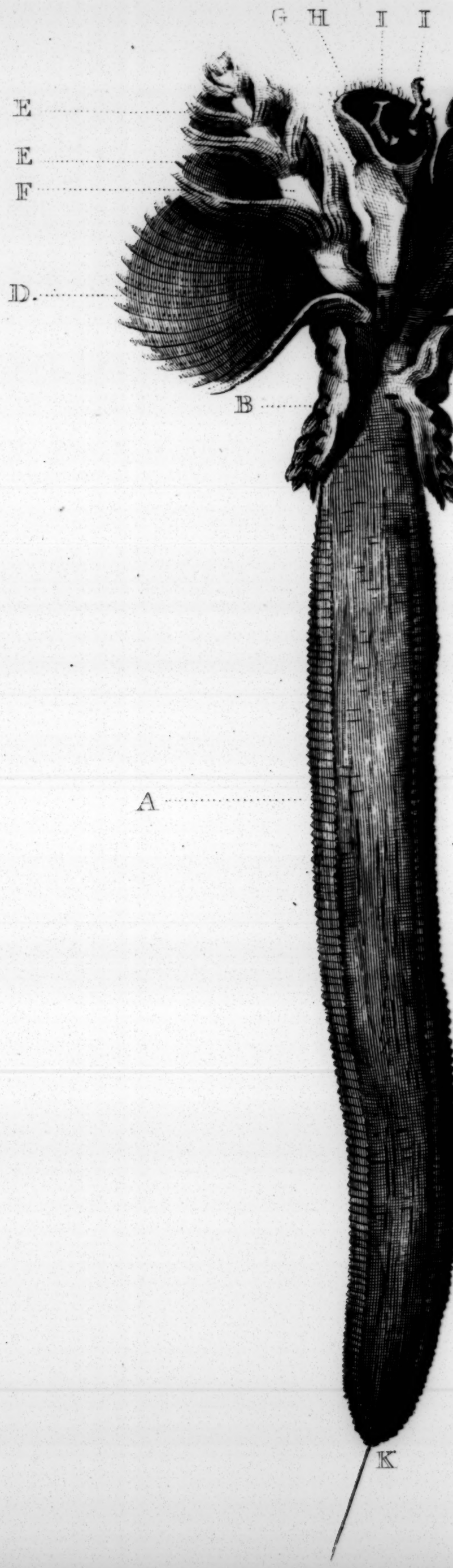

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Fig. I.

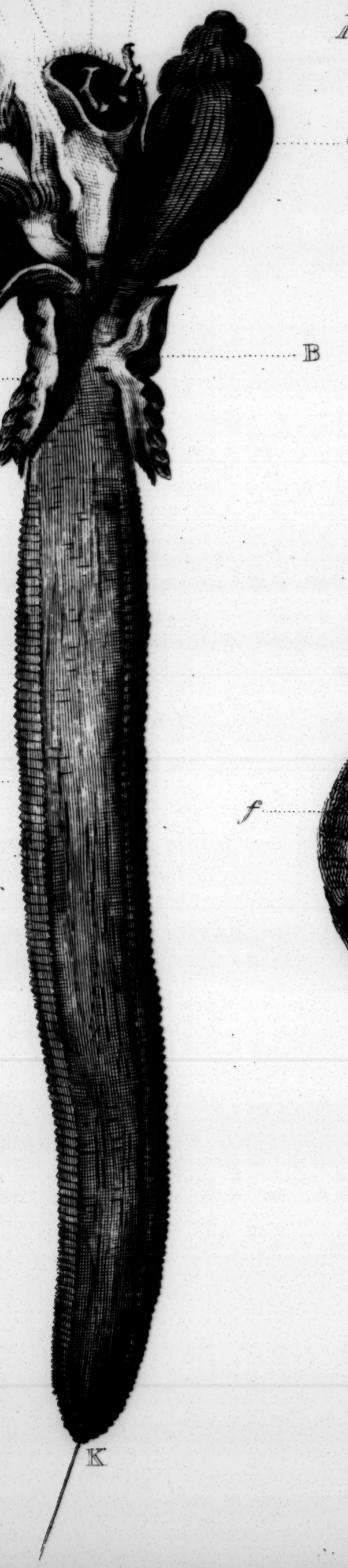
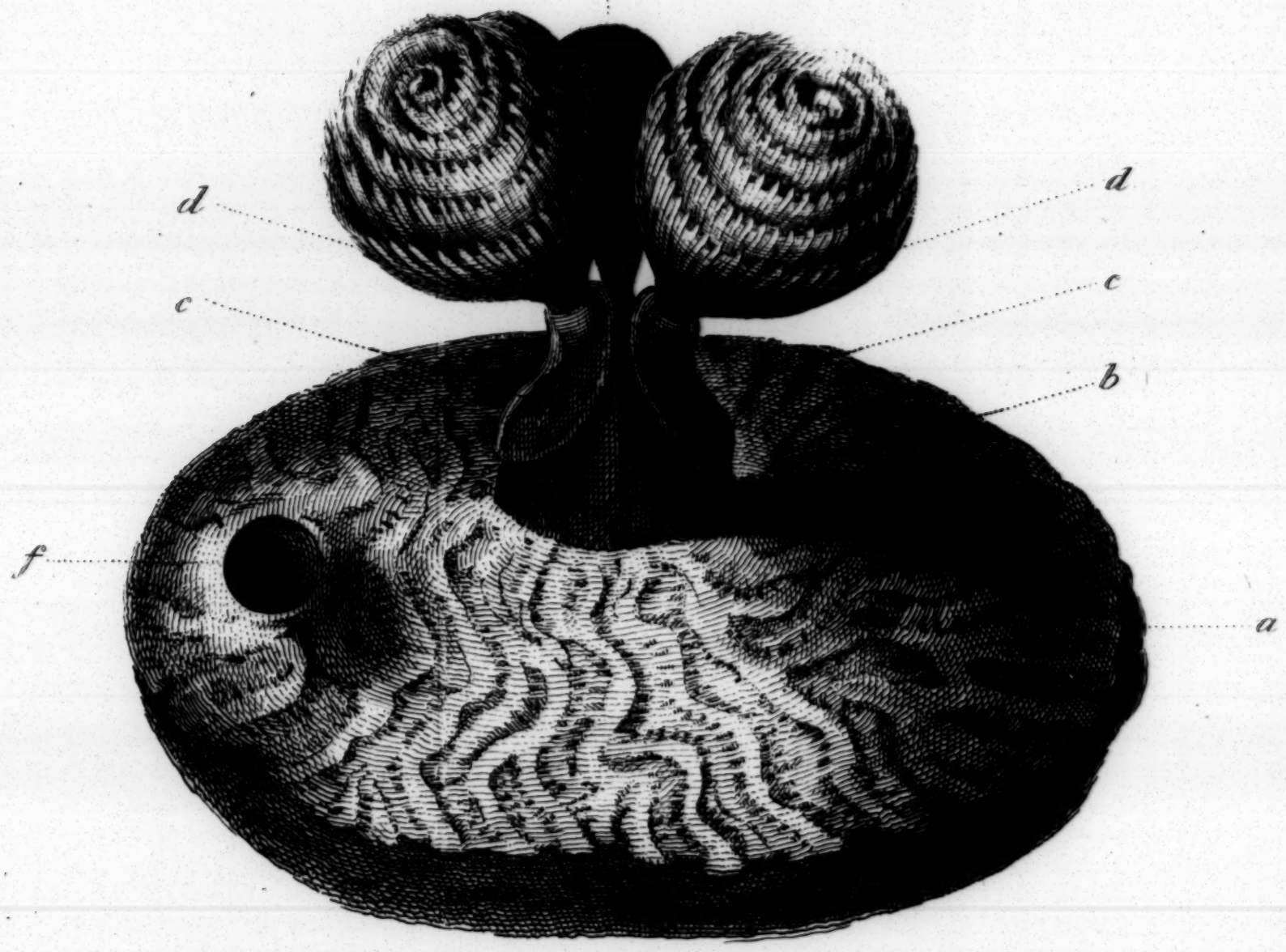


Fig. II.



while the animal was alive. This alſo is larger than the animal.

a. The ſort of brain-ſtone in which the animal was diſcovered.

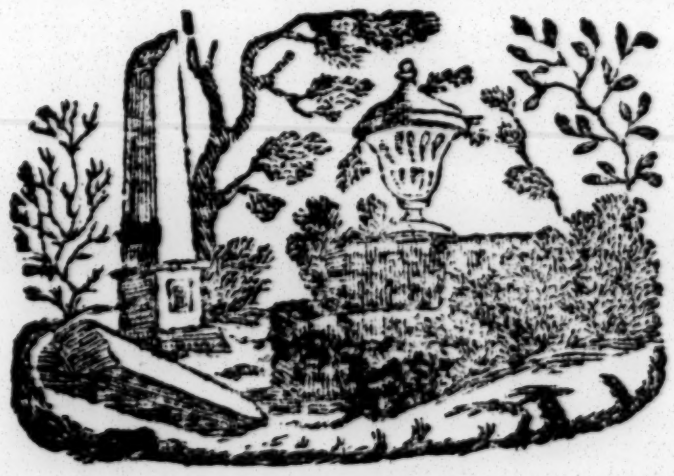
b. The external prominent ſhell.

cc. The membrane which is protruded with the cones and moveable ſhell, and makes a fold over the edges of the prominent ſhell.

dd. The membranes and tentacula in a ſtate of expanſion.

e. The inner ſide of the moveable ſhell, as it appears when protruded.

f. The hole in the brain-ſtone as it appears when the prominent ſhell is broken off, and which may be ſeen in many ſpecimens of brain-ſtone.



XVIII. *A Deſcription of a new Syſtem of Wires in the Focus of a Teleſcope, for obſerving the comparative Right Aſcenſions and Declinations of cœleſtial Objects; together with a Method of inveſtigating the ſame when obſerved by the Rhombus, though it happen not to be truly in an equatorial Poſition.* By the Rev. Francis Wollaſton, *LL.B. F.R.S.*

Read April 7, 1785.

IN conſequence of a paper communicated the laſt year to this Society, and honoured with a place in our Tranſactions, it may be expected of me, that I ſhould now deliver in an account of what farther obſervations I have made on that conſtellation of which I then gave a rough map. This I readily would do, if they were in any degree worthy of the Society's notice. But as yet they are far from perfect: how much better they may ſucceed hereafter, time muſt ſhew.

Yet has this year perhaps not quite been loſt; the difficulties which diſappointed my hopes, having led to what appears to me an improvement in the inſtrument with which to purſue ſuch obſervations.

My deſign, as was hinted in that Paper, was to aſcertain, as well as I was able, the right aſcenſions and declinations of the ſtars I had laid down; by obſerving their *meridian paſſages* and *meridian altitudes*, where that could be done with ſuch ſmall inſtruments as mine; as alſo by their *comparative paſſages* through the field of an equatorial teleſcope furniſhed with a

ſyſtem of wires invented by Dr. BRADLEY, and called by the French *Reticule Rhomboïde*, whence it has commonly obtained in Engliſh the name of the Rhomboid.

In the former I was diſappointed by the weather; which from the time I went into the country, in the middle of May, till the end of June, when that conſtellation came to the meridian in the day-light, afforded me very few evenings fit for obſervation.

In the latter I failed, through the imperfection of my inſtrument, or my own want of ſkill in the uſe of it; for though a ſingle ſet of obſervations in any one evening would appear very good, yet when reduced by calculation, and confronted with other repeated trials, they never gave me the ſatisfaction I wiſhed.

The rhombus (for a rhombus, and not a rhomboid, it ought moſt properly to be called) is very good in theory; but very difficult to get executed with preciſion, and liable to ſome inaccuracy in the obſervation. The truth of it depends upon the longer diagonal being exactly twice the length of the ſhorter one; which requires an aukward angle ($53^\circ\ 7'\ 48''$) at the vertex, not eaſily to be hit by the workmen, and therefore ſeldom ſufficiently true. Beſide this, as the ſides of the rhombus, on which depends the calculation for differences of declination, are but $26^\circ\ 33'\ 54''$ declining from the perpendicular or horary wire, a very ſmall error in obſerving the paſſage of a ſtar makes a very material difference in the reſult.

This determined me upon making trial of a ſquare placed angularly (an addition to M. CASSINI's wires at 45°, as may be ſeen in tab. XII. fig. 1.) which ſeems to anſwer better. I muſt confeſs I have not yet had opportunity for trying it ſo completely as I could wiſh: but I was unwilling to let this

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year ſlip by, without making it known; ſince, I think, from what I have done with it, I may be confident of its utility *.

The *properties* and *advantages* of ſuch a ſyſtem of wires ſcarcely need to be pointed out to aſtronomers. The whole extent of the field is employed as it is in the rhombus (the want of which was ſaid to be Dr. BRADLEY's objection to M. CASSINI's wires); but being formed of right angles or half-right angles, to which workmen are moſt accuſtomed, they will always be apt to execute their part better; and the obliques, from the differences being juſt double to what they are in the rhombus, give the comparative declinations with twice the certainty. To this the number of correſponding obſervations in the paſſage of every ſtar add conſiderably; ſince you may calculate its diſtance from the center C, from the angle D or E, or from one of the intermediate angles K, as you ſhall ſee occaſion. The ſame indeed you may do in the rhombus from D or from E; or, if the rhombus be formed of wires, from the angle at L, fig. 2.; but only with half the preciſion. The reſult of a ſingle paſſage of any one ſtar (excepting towards the extremities of the field) gives the extent of the field equally in each, provided the declination of the ſtar be known, by deducing its diſtance from thoſe ſeveral angles; and ſuch deductions ſerve as a ſtill farther check upon every obſervation; be-

* What is here offered is by no means to be underſtood as recommending any ſyſtem of wires in preference to actual meaſurement with a micrometer, but to render the uſe of them as convenient as may be to ſuch gentlemen as are not provided with better inſtruments. The equatorial micrometer with a large field (ſuch as I have ſeen at Mr. AUBERT's, of Mr. SMEATON's conſtruction) I take to be the beſt inſtrument for taking differences of right aſcenſion and declination out of the meridian; and far ſuperior to any ſyſtem of fixed wires, or indeed to any equatorial ſector whatever.

cauſe, if any part of it be thought doubtful, its tallying or not tallying with the known extent of the field will ſhew whether there be any error, or where it lies. And, in each of them, the parallel wires will tell you whether the placing of your inſtrument be true or faulty; becauſe, if truly made and truly ſet, the ſame ſtar muſt take the ſame time in paſſing from one wire to its correſponding parallel; which will differ conſiderably, and in every ſtar the ſame way, if the poſition be faulty.

Some of theſe latter remarks might have been ſpared, but that they may ſerve as hints to ſuch gentlemen as may be inclined to lend their aſſiſtance to what was propoſed the laſt year, and who may not have conſidered the many helps to be derived from a croſs examination of the obſervations they make. For their uſe alſo it may be proper to add, what indeed is nothing new, that if the poſition of the inſtrument be found erroneous, the formula given by M. DE LA LANDE in his Aſtronomy will ſerve to rectify the obſervation. Calling the larger interval between the paſſage of any oblique and the horary wire *m*, and the ſmaller one *n*, $\frac{m^2n + n^2m}{m^2 + n^2}$ will give the difference of declination (in time, to be converted into degrees, and multiplied by the coſine of declination) from the angle where that oblique meets the horary; and $\frac{m^2n - n^2m}{m^2 + n^2}$ the difference in right aſcenſion from the ſame angle. It muſt ſurely be almoſt needleſs to mention, that where the poſition is true, *half* the interval of time between a ſtar's paſſing any two correſponding obliques, converted into degrees, and multiplied by the coſine of declination, will give the difference in declination of that ſtar from the angle where thoſe obliques meet, as the *whole* interval does in the rhombus.

But

But it may, perhaps, be of ſervice to aſtronomy, or at leaſt not unacceptable to thoſe gentlemen who uſe the Rhombus, that I ſhould ſubjoin another formula (contrived for me the laſt ſummer by my Son, now Mathematical Lecturer at Sidney College, Cambridge) for inveſtigating the comparative right aſcenſions and declinations of ſtars obſerved by it, when the inſtrument is not placed truly in the plane of the equator. I was led into wiſhing for ſome ſuch formula, in conſequence of an ingenious Paper, kindly communicated to me by Sir H. C. ENGLEFIELD, Bart. F. R. S. giving an account of his method of doing it by a ſcale and figure; which, though very eaſy when one is provided with ſuch a ſcale, appeared to me to be of leſs general uſe than by calculation; and I do not know that any thing of the kind is to be met with in any publication.

Let the angle DLL, fig. 2. (which, by conſtruction, is 63° 26′ 6″) be called - - - - - *a*

The diagonal LL (whoſe extent, that is, what portion of a great circle it comprehends, muſt be known to the obſerver) be called - - - - - - *b*

The larger interval obſerved between the paſſage of a ſtar by an oblique and the horary wire (as *b c*) - - *m*

The ſmaller ditto of the ſame ſtar (as *c d*) - *n*

The larger ditto of another ſtar (as $\beta\gamma$) - - μ

The ſmaller ditto (as $\gamma\delta$) - - - ν

Then $\frac{2 \cdot \overline{m-n}}{m+n}$ = tangent of the angle which LL makes with a parallel of declination: call this *q*

The angle *q* being thus found, then

$\frac{2 \cdot \overline{n \sim \nu} \times \text{ſin.} \overline{a+q}}{\text{R} \times \text{ſin.}\ a} \times \text{coſ.}\ q.$ = difference in declination between the two points on the vertical wire where thoſe

thoſe ſtars paſs it. N. B. This being in *time* muſt be converted into *degrees*, and multiplied by coſine of declination as uſual, to give the true difference in declination between the ſtars.

And the ſame expreſſion, *viz.*

$\frac{2 \cdot n \backsim \nu \times \text{fin.} \overline{a+q}}{R \times \text{fin.} a} \times \text{fin.}\ q =$ the difference in $\mathcal{R}$ between thoſe two points; to be applied as a correction to the obſerved times.

The ſame may be done by the larger intervals m and μ, only by ſubſtituting $\overline{a-q}$ in the place of $\overline{a+q}$, thus:

$\frac{2 \cdot m \backsim \mu \times \text{fin.} \overline{a-q}}{R \times \text{fin.} a} \times \text{cof.}\ q =$ difference in declination as above;

or . . . . . $\times \text{fin.}\ q =$ aſcenſional difference.

If the ſtars differ too much in declination to come within the expreſſion above (as N° 2. and 3.) then the differences of the angles D and E in declination and right aſcenſion may be found thus:

$\frac{2 \cdot b \times \text{cof.}\ q}{R} =$ difference in declination between D and E;

$\frac{2 \cdot b \times \text{fin.}\ q}{R} =$ their aſcenſional difference;

and the difference of each ſtar from its reſpectively neareſt angle of the rhombus, may be deduced by the former expreſſion, leaving out the conſideration of the other ſtar, thus:

$\frac{2 \cdot n \times \text{fin.} \overline{a+q}}{R \times \text{fin.} a} \times \text{cof.}\ q =$ difference of the ſtar in declination from its neareſt angle.

and . . . $\times \text{fin.}\ q =$ its difference in right aſcenſion.

The application of theſe formulæ is very eaſy: for having ound q, if you ſet down its coſine in one column for declination.

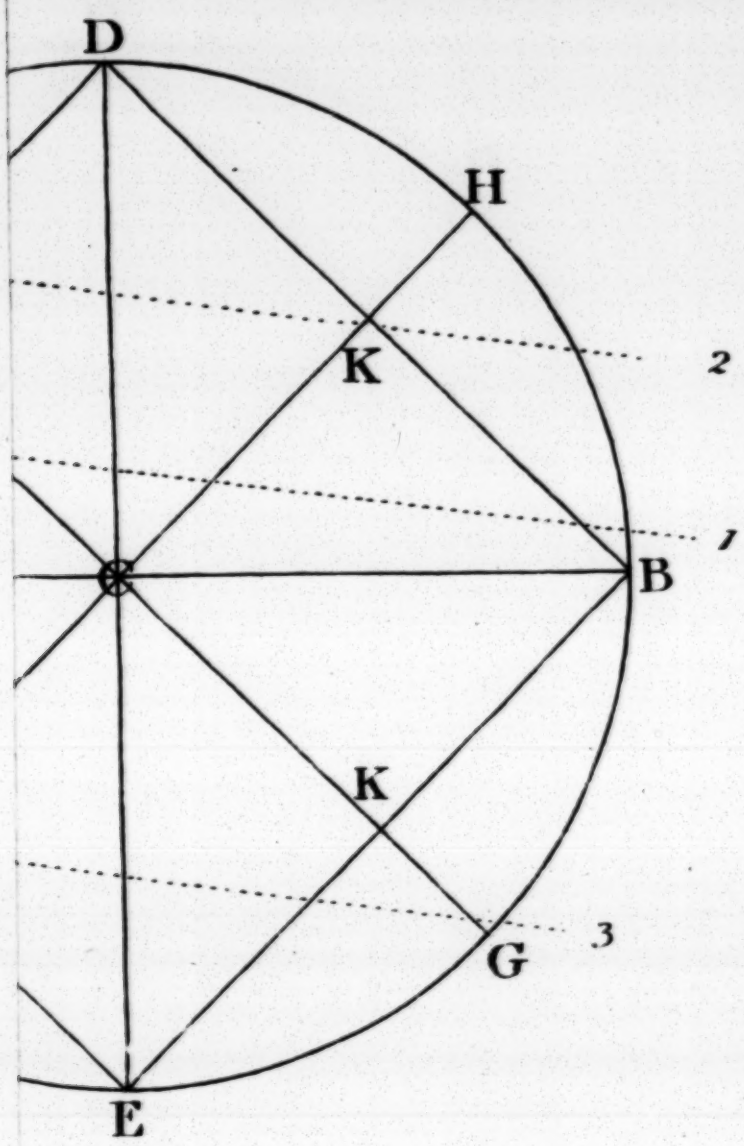
tion, and its ſine in another column for right aſcenſion, and under each the conſtant ſin. $\overline{a+q}$, and the arithmetical compl. of ſin. a; theſe being added together will make two ſums, for the comparative obſervations of every ſtar which may paſs your field; and, unleſs your field be very large, and the declination of the ſtars very great, if to the column for declination you add the coſine of declination of the center of your field, it will adapt itſelf to all the products.

FRANCIS WOLLASTON.

Charter-houſe-Square,
March 15, 1785.

POSTSCRIPT.

SINCE the delivery of this Paper, it has occurred to me, that it may ſometimes be convenient to know the angle of deviation from the true equatorial poſition in the new ſyſtem of wires. This is to be deduced nearly in the ſame manner as in the rhombus; for $\frac{m-n}{m+n}$ = tang. q. By this angle any obſerved differences in right aſcenſion may be corrected: for the difference in declination between any two ſtars (or their difference from the angle) multiplied by ſin. q, will give the correction required.


D
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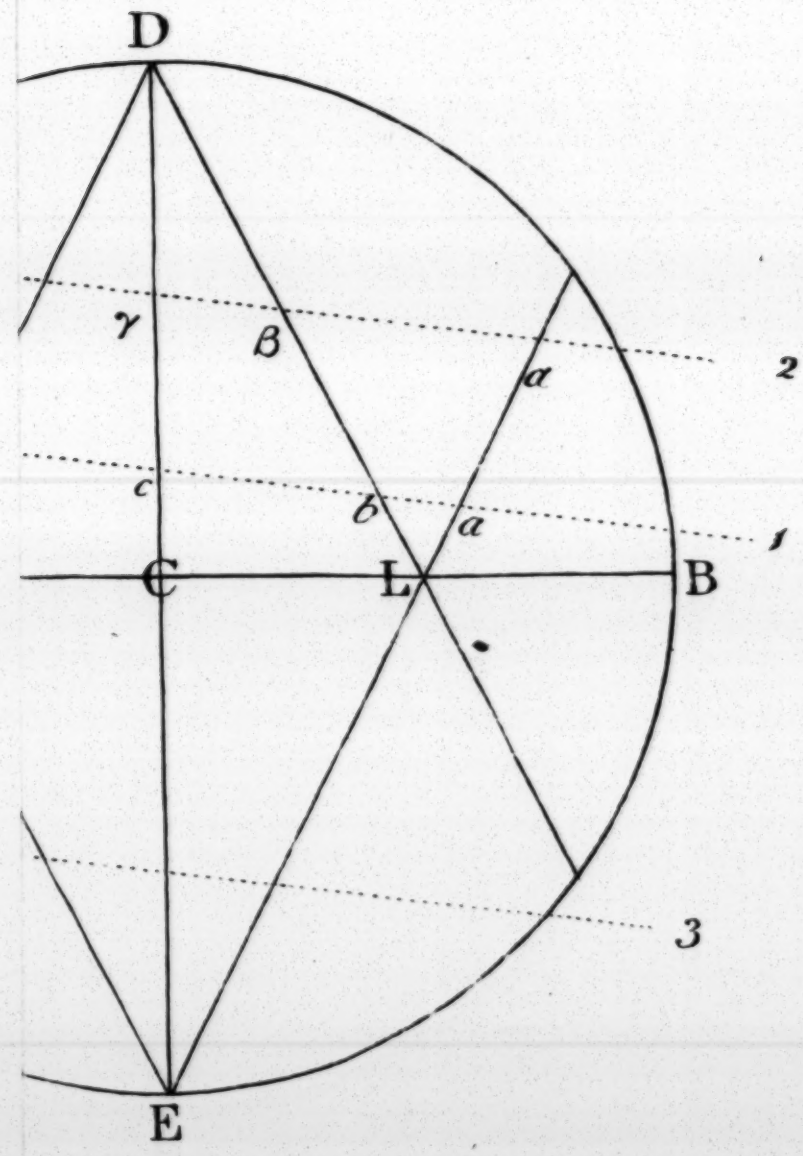
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Fig. 1.

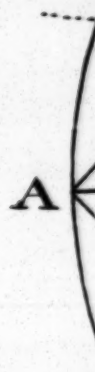


Fig. 2.

Fig. 1.

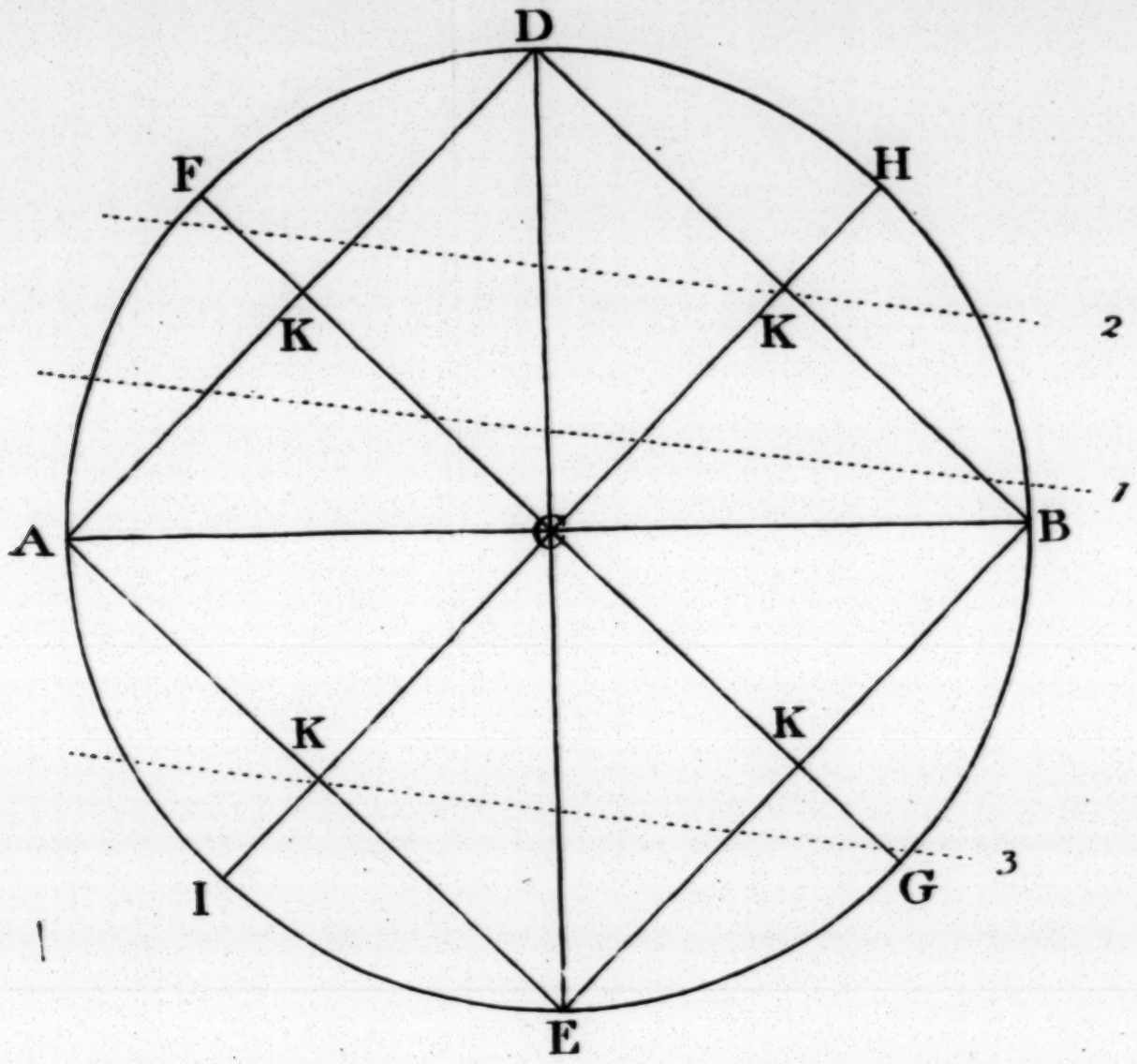
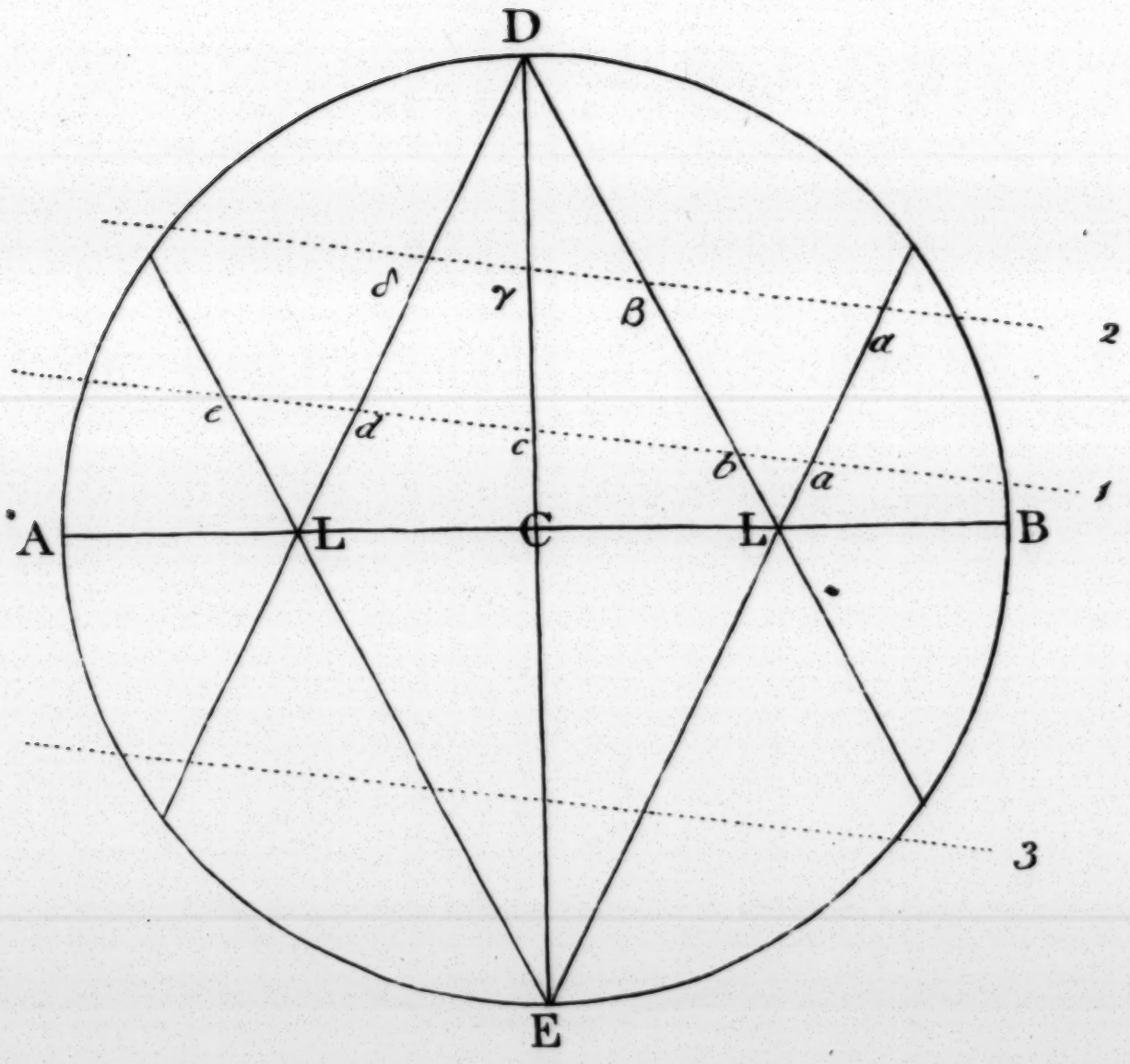


Fig. 2.



XI

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XIX. *An Account of a Stag's Head and Horns, found at* Alport, *in the Pariſh of* Youlgreave, *in the County of* Derby. *In a Letter from the Rev.* Robert Barker, *B.D. to* John Jebb, *M.D. F.R.S.*

Read April 14, 1785.

ABOUT five years ago, ſome men working in a quarry of that kind of ſtone which in this part of the country we call tuft *, at about five or ſix feet below the ſurface, in a very ſolid part of the rock, met with ſeveral fragments of the horns and bones of one or different animals. Amongſt the reſt, out of a large piece of the rock, which they got entire, there appeared the tips of three or four horns, projecting a few inches from it, and the ſcapula of ſome animal adhering to the outſide of it. A friend of mine, to whom the quarry belongs, ſent the piece of the rock to me in the ſtate they got it, in which I let it remain for ſome time. But ſuſpecting that they might be tips of the horns of ſome head encloſed in the lump, I determined to gratify my curioſity by clearing away the ſtone from the horns. On doing which I found that the lump contained a very large ſtag's head, with two antlers upon each horn, in very perfect preſervation, incloſed in it.

* Tuft is a ſtone formed by the depoſit left by water paſſing through beds of ſticks, roots, vegetables, &c. of which there is a large ſtratum at Matlock Bath, in this county.

Though the horns are ſo much larger than thoſe of any ſtag I have ever ſeen, yet, from the ſutures in the ſkull appearing very diſtinct in it, one would ſuppoſe that it was not the head of a very old animal. I have one of the horns nearly entire, and the greateſt part of the other, but ſo broken in the getting out of the rock, that one part will not join to the other, as the parts of the other horn do. The horns are of that ſpecies which park-keepers in this part of the country call throſtle-neſt horns, from the peculiar formation of the upper part of them, which is branched out into a number of ſhort antlers which form an hollow about large enough to contain a thruſh's neſt. I ſend you the dimenſions of the different parts of them, compared with the horns of the ſame ſpecies of a large ſtag, which have probably hung in the place from whence I procured them two or three or perhaps more centuries; and with another pair of horns of a different kind, which are terminated by one ſingle pointed antler, and which were the horns of a ſeven-year-old ſtag.

The river Larkell runs down the valley, and part of it falls into the quarry where theſe horns were found, the water of which has not the property of incruſting any bodies it paſſes through. It is therefore probable, that the animal to which theſe horns belonged was waſhed into the place where they were found, at the time of ſome of thoſe convulſions which contributed to raiſe this part of the iſland out of the ſea. Beſides this complete head, I have ſeveral pieces of horns, bones (particularly the ſcapula I mentioned above), and ſeveral vertebræ of the back, found in the ſame quarry; ſome, if not all, of them probably belonging to the animal whoſe head is in my poſſeſſion.

Dimen-

Dimenſions of the horns found at Alport.

	Ft.	In.
Circumference at their inſertion into the corona, .	0	$9\frac{7}{8}$
Length of the loweſt antler, . . .	1	2
Length of ſecond ditto, . . .	0	$11\frac{1}{2}$
Length of third ditto, . . .	1	$1\frac{1}{2}$
Length of the horn, . . .	3	$3\frac{1}{2}$

Dimenſions of a large pair of throſtle-neſt horns.

Circumference at their inſertion into the corona, .	0	7
Length of the loweſt antler, . . .	1	0
Length of ſecond ditto, . . .	0	$10\frac{5}{8}$
Length of third ditto, . . .	0	$11\frac{1}{2}$
Length of the horn, ...	2	$7\frac{1}{2}$

Dimenſions of the horns of a ſtag ſeven years old.

Circumference at their inſertion into the corona, .	0	$5\frac{1}{2}$
Length of the loweſt antler, . . .	0	9
Length of ſecond ditto, . . .	0	10
Length of third ditto, . . .	0	10
Length of the horn, ...	2	$8\frac{3}{4}$

Youlgreave, Jan. 23, 1785.

XX. *An Account of the ſenſitive Quality of the Tree* Averrhoa Carambola. *In a Letter from* Robert Bruce, *M.D. to Sir* Joſeph Banks, *Bart. P.R.S.*

Read April 14, 1785.

THE Averrhoa Carambola of LINNÆUS, a tree called in Bengal the Camruc or Camrunga, is poſſeſſed of a power ſomewhat ſimilar to thoſe ſpecies of Mimoſa which are termed ſenſitive plants; its leaves, on being touched, move very perceptibly.

In the Mimoſa the moving faculty extends to the branches; but, from the hardneſs of the wood, this cannot be expected in the Camrunga. The leaves are alternately pinnated, with an odd one; and in their moſt common poſition in the day-time are horizontal, or on the ſame plane with the branch from which they come out. On being touched, they move themſelves downwards, frequently in ſo great a degree that the two oppoſite almoſt touch one another by their under ſides, and the young ones ſometimes either come into contact or even paſs each other.

The whole of the leaves of one pinna move by ſtriking the branch with the nail of the finger, or other hard ſubſtance; or each leaf can be moved ſingly, by making an impreſſion that ſhall not extend beyond that leaf. In this way, the leaves of one ſide of the pinna may be made to move, one after another, whilſt the oppoſite continue as they were; or you may make them

them move alternately, or, in ſhort, in any order you pleaſe, by touching in a proper manner the leaf you wiſh to put in motion. But if the impreſſion, although made on a ſingle leaf, be ſtrong, all the leaves on that pinna, and ſometimes on the neighbouring ones, will be affected by it.

What at firſt ſeemed ſurpriſing was, that notwithſtanding this apparent ſenſibility of the leaf, I could with a pair of ſharp ſciſſars make large inciſions in it, without occaſioning the ſmalleſt motion; nay, even cut it almoſt entirely off, and the remaining part ſtill continue unmoved; and that then, by touching the wounded leaf with the finger or point of the ſciſſars, motion would take place as if no injury had been offered. But, on further examination, I found, that although the leaf was the oſtenſible part which moved, it was in fact entirely paſſive, and that the petiolus was the ſeat both of ſenſe and action: for although the leaf might be cut in pieces, or ſqueezed with great force, provided its direction was not changed, without any motion being occaſioned; yet, if the impreſſion on the leaf was made in ſuch a way as to affect the petiolus, the motion took place. When, therefore, I wanted to confine the motion to a ſingle leaf, I either touched it ſo as only to affect its own petiolus, or, without meddling with the leaf, touched the petiolus with any ſmall-pointed body, as a pin or knife.

By compreſſing the univerſal petiolus near the place where a partial one comes out, the leaf moves in a few ſeconds, in the ſame manner as if you had touched the partial petiolus.

Whether the impreſſion be made by puncture, percuſſion, or compreſſion, the motion does not inſtantly follow; generally ſeveral ſeconds intervene, and then it is not by a jirk, but regular.

regular and gradual. Afterwards, when the leaves return to their former ſituation, which is commonly in a quarter of an hour or leſs, it is in ſo ſlow a manner as to be almoſt imperceptible.

On ſticking a pin into the univerſal petiolus at its origin, the leaf next it, which is always on the outer ſide, moves firſt; then the firſt leaf on the oppoſite ſide, next the ſecond leaf on the outer, and ſo on. But this regular progreſſion ſeldom continues throughout; for the leaves on the outer ſide of the pinna ſeem to be affected both more quickly, and with more energy, than thoſe of the inner, ſo that the fourth leaf on the outer ſide frequently moves as ſoon as the third on the inner; and ſometimes a leaf, eſpecially on the inner ſide, does not move at all, whilſt thoſe above and below it are affected in their proper time. Sometimes the leaves at the extremity of the petiolus move ſooner than ſeveral others which were nearer the place where the pin was put in.

On making a compreſſion with a pair of pincers on the univerſal petiolus, between any two pair of leaves, thoſe above the compreſſed part, or nearer the extremity of the petiolus, move ſooner than thoſe under it, or nearer the origin; and frequently the motion will extend upwards to the extreme leaf, whilſt below it perhaps does not go farther than the neareſt pair.

If the leaves happen to be blown by the wind againſt one another, or againſt the branches, they are frequently put in motion; but when a branch is moved gently, either by the hand or the wind, without ſtriking againſt any thing, no motion of the leaves takes place.

When left to themſelves in the day-time, ſhaded from the ſun, wind, rain, or any diſturbing cauſe, the appearance of

the leaves is different from that of other pinnated plants. In the laſt a great uniformity ſubſiſts in the reſpective poſition of the leaves on the pinna; but here ſome will be ſeen on the horizontal plane, ſome raiſed above it, and others fallen under it; and in an hour or ſo, without any order or regularity, which I could obſerve, all theſe will have changed their reſpective poſitions. I have ſeen a leaf, which was high up, fall down; this it did as quickly as if a ſtrong impreſſion had been made on it, but there was no cauſe to be perceived.

Cutting the bark of the branch down to the wood, and even ſeparating it about the ſpace of half an inch all round, ſo as to ſtop all communication by the veſſels of the bark, does not for the firſt day affect the leaves, either in their poſition or their aptitude for motion.

In a branch, which I cut through in ſuch a manner as to leave it ſuſpended only by a little of the bark no thicker than a thread, the leaves next day did not riſe ſo high as the others; but they were green and freſh, and, on being touched, moved, but in a much leſs degree than formerly.

After ſun-ſet the leaves go to ſleep, firſt moving down ſo as to touch one another by their under ſides; they therefore perform rather more extenſive motion at night of themſelves than they can be made to do in the day-time by external impreſſions. With a convex lens I have collected the rays of the ſun on a leaf, ſo as to burn a hole in it, without occaſioning any motion. But when the experiment is tried on the petiolus, the motion is as quick as if from ſtrong percuſſion, although the rays were not ſo much concentrated as to cauſe pain when applied in the ſame degree on the back of the hand; nor had the texture of the petiolus been any ways changed by this; for next day it

could not be diſtinguiſhed, either by its appearance or moving power, from thoſe on which no experiment had been made.

The leaves move very faſt from the electrical ſhock, even although a very gentle one; but the ſtate of the atmoſphere was ſo unfavourable for experiments of this kind, that I could not purſue them ſo far as I wiſhed.

There are two other plants mentioned as ſpecies of this genus by LINNÆUS. The firſt, the Averrhoa Bilimbi, I have not had an opportunity of ſeeing. The other, or Averrhoa Acida, does not ſeem to belong to the ſame claſs; nor do its leaves poſſeſs any of the moving properties of the Carambola. LINNÆUS's generic deſcription of the Averrhoa, as of many other plants in this country which he had not an opportunity of ſeeing freſh, is not altogether accurate. The petals are connected by the lower part of the lamina, and in this way they fall off whilſt the ungues are quite diſtinct. The ſtamina are in five pairs, placed in the angles of the germen. Of each pair only one ſtamen is fertile, or furniſhed with an anthera. The filaments are curved, adapted to the ſhape of the germen. They may be preſſed down gently, ſo as to remain; and then, when moved a little upwards, riſe with a ſpring. The fertile are twice the length of thoſe deſtitute of antheræ.

Calcutta, Nov. 23, 1783.

XXI. *An Account of ſome Experiments on the Loſs of Weight in Bodies on being melted or heated. In a Letter from* George Fordyce, *M.D. F.R.S. to Sir* Joſeph Banks, *Bart. P.R.S.*

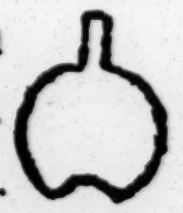
Read April 28, 1785.

SIR,

ALTHOUGH I have made many experiments on the ſubject of the loſs of weight in bodies on being melted, or heated, I do not think it worth while to lay them all before the Society, as there has not appeared any circumſtance of contradiction in them. I ſhall content myſelf with relating the following one, which appears to me concluſive in determining the loſs of weight in ice when thawed into water, and ſubject to the leaſt fallacy of any I have hitherto made, in ſhewing the loſs of weight in ice on being heated.

The beam I made uſe of was ſo adjuſted as that, with a weight between four and five ounces in each ſcale, $\frac{1}{1600}$ part of a grain made a difference of one diviſion on the index. It was placed in a room, the heat of which was 37 degrees of FAHRENHEIT's thermometer, between one and two in the afternoon, and left till the whole apparatus and the braſs weights acquired the ſame temperature.

A glaſs globe, of three inches diameter nearly, with an indentation at the bottom, and a tube at the top, weighing about 451 grains, had about 1700 grains of *New-River* water poured into it, and was hermetically ſealed, ſo that the



whole, when perfectly clean, weighed 2150$\frac{5}{12}$ of a grain exactly; the heat being brought to 32 degrees, by placing it in a cooling mixture of ſalt and ice till it juſt began to freeze, and ſhaking the whole together.

After it was weighed it was again put into the freezing mixture, and let ſtand for about twenty minutes; it was then taken out of the mixture; part of the water was found to be frozen; and it was carefully wiped, firſt with a dry linen cloth, and afterwards with dry waſhed leather; and on putting it into the ſcale it was found to have gained about the $\frac{1}{60}$ part of a grain. This was repeated five times: at each time more of the water was frozen, and more weight gained. In the mean time the heat of the room and apparatus had ſunk to the freezing point.

When the whole was frozen, it was carefully wiped and weighed, and found to have gained $\frac{3}{160}$ of a grain and four diviſions of the index. Upon ſtanding in the ſcale for about a minute, I found it began to loſe weight, on which I immediately took it out, and placed it at a diſtance from the beam. I alſo immediately plunged a thermometer in the freezing mixture, and found the temperature 10 degrees; and on putting the ball of the thermometer in the hollow at the bottom of the glaſs veſſel, it ſhewed 12 degrees. I left the whole for half an hour, and found the thermometer, applied to the hollow of the glaſs, at 32°. Every thing now being at the ſame temperature, I weighed the glaſs containing the ice, after wiping it carefully, and found it had loſt $\frac{1}{3}$ and five diviſions; ſo that it weighed $\frac{1}{160}$, all but one diviſion, more than when the water was fluid.

I now melted the ice, excepting a very ſmall quantity, and left the glaſs veſſel expoſed to the air in the temperature of 32

degrees

degrees for a quarter of an hour; the little bit of ice continued nearly the ſame. I now weighed it, after carefully wiping the glaſs, and found it heavier than the water was at firſt one diviſion of the beam. Laſtly, I took out the weights, and found the beam exactly balanced as before the experiment.

The acquiſition of weight found on water's being converted into ice, may ariſe from an increaſe of the attraction of gravitation of the matter of the water; or from ſome ſubſtance imbibed through the glaſs, which is neceſſary to render the water ſolid.

Which of theſe poſitions is true may be determined, by forming a pendulum of water, and another of ice, of the ſame length, and in every other reſpect ſimilar, and making them ſwing equal arcs. If they mark equal times, then certainly there is ſome matter added to the water. If the pendulum of ice is quicker in its vibrations, then the attraction of gravitation is increaſed. For there is no poſition more certain, than that a ſingle particle of inanimate matter is perfectly incapable of putting itſelf in motion, or bringing itſelf to reſt; and therefore, that a certain force applied to any maſs of matter, ſo as to give it a certain velocity, will give half the quantity of matter double the velocity, and twice the quantity, half the velocity; and, generally, a velocity exactly in the inverſe proportion to the quantity of matter. Now, if there be the ſame quantity of matter in water as there is in ice, and if the force of gravity in water be $\frac{1}{28000}$ part leſs than in ice, and the pendulum of ice ſwing ſeconds, the pendulum of water will loſe $\frac{1}{28000}$ of a ſecond in each vibration, or one ſecond in 28000, which is almoſt three ſeconds a day, a quantity eaſily meaſured.

I ſhall juſt take notice of an opinion which has been adopted by ſome, that there is matter abſolutely light, or which repels inſtead of attracting other matter. I confeſs this appears abſurd to me; but the following experiment would prove or diſprove it. Suppoſing, for inſtance, that heat was a body, and abſolutely light, and that ice gained weight by loſing heat; then a pendulum of ice would ſwing through the ſame arc in $\frac{1}{14000}$ leſs time than a ſimilar pendulum of water; for the ſame power would not only act upon a leſs quantity of matter, but a counter-acting force would alſo be taken away.

Till the experiment of the pendulum can be made, or ſome other equally certain be ſuggeſted and made, it would be waſting time to enter into conjecture about the cauſe of the gain of weight in the converſion of water into ice in a glaſs veſſel hermetically ſealed.

I ſhall only obſerve, that heat certainly diminiſhes the attractions of coheſion, chemiſtry, magnetiſm, and electricity; and if it ſhould alſo turn out, that it diminiſhes the attraction of gravitation, I ſhould not heſitate to conſider heat as the quality of diminution of attraction, which would in that caſe account for all its effects.

We come, in the next place, to take notice of the ſecond part of the experiment, *viz.* that the ice gained an eighth part of a grain on being cooled to 12 degrees of FAHRENHEIT's thermometer. In this caſe, a variation may ariſe from the contraction of the glaſs veſſel, and conſequent increaſe of ſpecific gravity in proportion to the air. But it is unneceſſary to obſerve, that this would be ſo very ſmall a quantity as not to be obſervable upon a beam adjuſted only to the degree of ſenſibility with which this experiment was tried. In the ſecond place, the air cooled by the ice above the ſcale becoming heavier than

the ſurrounding atmoſphere, would preſs upon the ſcale downward with the whole force of the difference. If a little more than half a pint of air was cooled over the ſcale to the heat of the ice and glaſs containing it, that is, twenty degrees below the freezing point, the difference, according to General Roy's table, would have been the eighth part of a grain, which was the weight acquired; but the air within half an inch of the glaſs veſſel being only one degree below the freezing point, I cannot conceive, that even an eighth part of a pint of air could be cooled over the ſcale to twenty degrees below the freezing point; nor that the whole difference of the weight of the air over the ſcale could ever amount to the 32d of a grain. I have, however, contrived an apparatus which is executing, in which this cauſe of fallacy will be totally removed. I ſhall, therefore, reſt at preſent the ſtate of this part of the ſubject; and leave it only proved, that water gains weight on being frozen.

I am, &c.

G. FORDYCE.

XXII. *Sketches and Deſcriptions of three ſimple Inſtruments for drawing Architecture and Machinery in Perſpective.* By Mr. James Peacock; *communicated by* Robert Mylne, *Eſq. F.R.S.*

Read March 17, 1785.

SOME of the following machines muſt be placed upon the front edge of the table upon which they are to ſtand.

The ſights may be ſupported by a three-legged ſtaff.

The ſtocks of the ſquares or indexes may have ſteel ſprings upon their edges, in order to keep them in any aſſigned part of the grooves in which they are to ſlide.

FIG. I. (TAB. XIII.)

ABCD a drawing board, to be fixed on a table or ſtand, &c. in a vertical poſition. AB a ſliding-piece for the top of the T ſquare, having a rebate therein to form a groove, as expreſſed by the dotted line. CD, ſliding-piece for the bottom of the ſquare, having a rebate therein to form a groove for the reception of the ſtock as deſcribed by the dotted line; this ſliding-piece to be of ſufficient length to receive and ſupport the ſaid ſtock when the blade of the ſquare is coincident with the lines KNFH or LNGI. E a hole to receive the arm or ſlider of the ſight-piece, to be conſtructed in the uſual manner. FGHI an opening forming the field of view for the prototype. KLMN a ſheet of paper fixed on the upper part of the board for the copy, the four inner lines whereof form and incloſe a ſpace of the

the ſame dimenſions as the field of view reſpectively. OP a ſteel ſliding-piece, equal in length to the diſtance KF or IN; at the lower end P is a ſteel arm terminating in a point; and at the upper end, at O, is a ſimilar arm, terminating with a braſs button, in the center of which is a ſharp ſteel pricker; the ſaid pricker and the point P are to be equi-diſtant from the edge of the blade of the ſquare: this arm O is to have the faculty of a ſpring, in order that the pricker may clear itſelf of the ſurface of the paper as ſoon as the finger quits the button, in the ſame manner as is uſual in the apparatus of large protractors. This ſliding ſteel-piece may be drawn out of the dove-tailed or rebated groove at pleaſure, and the T ſquare will then be fit for ordinary uſes.

To uſe the Inſtrument.

Having fixed the board truly level and perpendicular, and placed the point of ſight, or hole for viſion, at ſuch a height and diſtance as ſhall be productive of the beſt effect, move the ſquare with one hand, and the ſteel ſlider with the other, until the point P coincides with the eye and any point or angle in the original object. Preſs the pricker at O, and the puncture will be the true place, or copy, of ſuch original point or angle, &c.

N. B. All perpendicular lines may be drawn at once (in pencil), by bringing the left-hand edge of the ſquare to coincide with the original line and the eye; and their lengths may be very nearly determined by the graduated edge of the ſquare, ſo as to prevent confuſion from unneceſſary lengths of lines. The ſaid graduated edge will alſo give the points in all curved or irregular objects.

FIG. II. (TAB. XIV.)

As the inſtrument, fig. 1. proceeds chiefly by finding the poſitions of Points, this is contrived to find the poſitions of Lines, and to determine their limits by their reciprocal interſections.

ABOCDE is a compound board, to be placed in a vertical poſition. FGHI is the opening, or field of view. KLMN is a looſe board, upon which paper is to be fixed; and the edges of the ſaid board are to be rebated, as deſcribed in the plan at *zz*. XYMN and OPQR are grooved receſſes, to receive the ſaid looſe board, as occaſion may require. STUW is a moveable parallelogram, compoſed of a rebated ſtock SU, two like-graduated rulers ST and UW, and the regulating piece TW; the whole connected with ſcrews, ſo as to move freely with a ſmall force; and the diſtances between the centers of motion SU or TW to be equal to KF or HQ. AE and ED are rebated grooves, in which the ſtock of the parallelogram is to move.

To uſe the Inſtrument.

Having fixed the compound board ABOCDE truly vertical, ſlip the papered board KLMN into the receſs XYMN, or OPQR, as the ſubject to be drawn may render firſt neceſſary, and ſlide the ſtock SU of the parallelogram into the groove AE, or ED, to correſpond therewith; then, by moving the ſtock in the groove with one hand, and at the ſame time regulating the parallelogram with the other, the top edge of the ruler UW may be brought to coincide with any line in the original object, and the figured diviſions on the edge of the ruler will at the ſame time determine the limits thereof, near enough to avoid a confuſion of unneceſſary lengths of line, &c. The true repre-

ſentation of the place and poſition of the line may be then drawn upon the paper, by the top edge of the ruler ST, a trifle longer at each extremity than it appears to be. This operation may be repeated for as many lines as can be obtained in the firſt poſition of the papered board and parallelogram; when they muſt be ſhifted into the other receſs and groove, to find the reſt, which may be now done without taking any further notice of the diviſions on the rulers.

N. B. A common T ſquare, applied to a board of this kind, will anſwer moſt purpoſes. For example: place the ſtock of ſuch a ſquare in one of the grooves, having a blade not leſs than the length HK or HR; mark the ſpaces HI and QR upon the upper edge thereof, and divide each of them into any convenient number of equal parts, and figure the ſaid parts in the uſual manner, to correſpond with each other, as may be ſeen in fig. 1. Now, ſuppoſe the ſtock of the ſquare to be in ED, it is plain, that all perpendicular lines may be drawn upon the paper KLMN in their proper places, and (by means of the diviſions on the edge of the ſquare) nearly of (though properly a trifle more than) their true length. All the lines of this deſcription being obtained, the ſhifting board muſt be placed in its other receſs, and the ſtock of the ſquare into the other groove; then, beginning with the firſt line, bring the edge of the ſquare to agree with its limits, and mark them off upon the line on the paper, and ſo of all the reſt in ſucceſſion; and join the points, where neceſſary, with a common ruler.

FIG. III. (TAB. XIV.)

This apparatus is contrived to avoid the trouble of continually working againſt a board in a vertical poſition. In order

to this, two ſquare boards are to be provided, equal in ſize, and of ſimilar conſtruction; one is to be fixed in a vertical poſition, for viewing the original object through a proper aperture; and the other is to be laid flat upon a deſk or table, for the greater eaſe and conveniency of drawing the copy upon paper to be fixed thereon for that purpoſe.

ABCD is the vertical board; EFGH the opening therein, forming the field of view; IKL the T ſquare, the blade thereof PL being moveable about the center P, with a moderate degree of ſtiffneſs; the ſtock K is to ſlide in a rebated or dove-tail groove AD, and be fixable to any part thereof by the ſcrew O; the ſteel points MN are to move with moderate eaſe in a rebated or dove-tail braſs groove in the middle of the blade of the ſquare; upon the back of the groove AD are to be fixed two braſs pins QQ, to reſt in proper holes, ſimilar to the holes marked RR; and the ſame kind of holes are to be made in the corner of the board whereon the copy is to be made.

To uſe the Machine.

Having placed the board ABCD in a truly vertical poſition, fix the ſhifting groove AD in the rebate, on the moſt convenient ſide of the board, by means of entering the pins Q into the holes R; then looſen the ſcrew O, and move the ſtock IK, and at the ſame time turn the blade PL upon its center P, until one of its edges ſhall be coincident with ſome original line; then fix the ſtock by turning the ſcrew O; move the points M and N, till they exactly include the apparent length of the ſaid line; then take off the ſhifting groove AD, together with the T ſquare or bevil fixed thereto, and apply the ſame to the

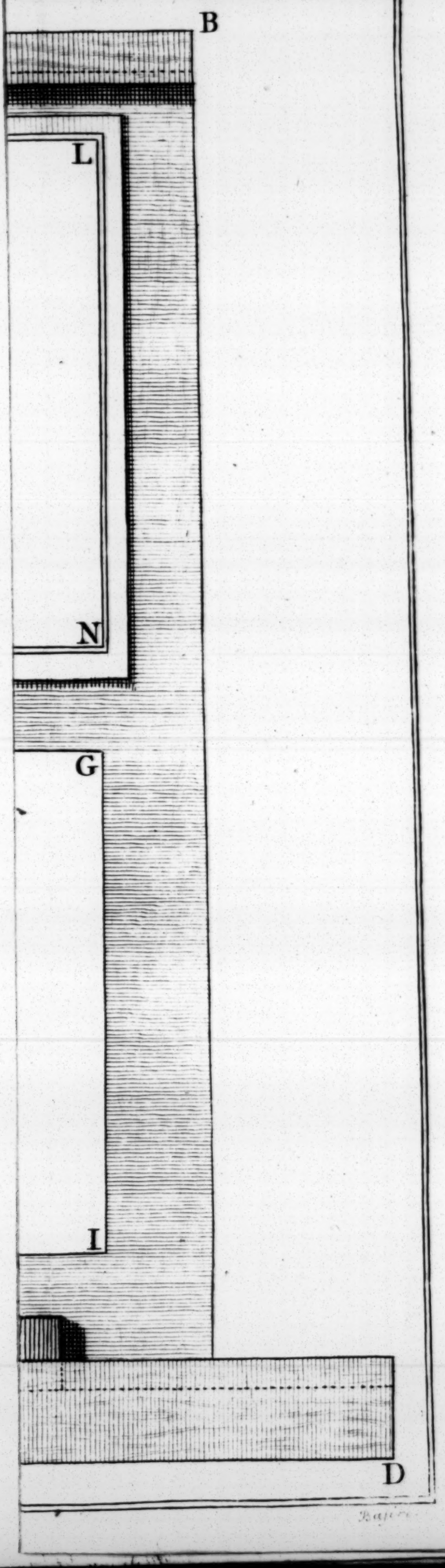

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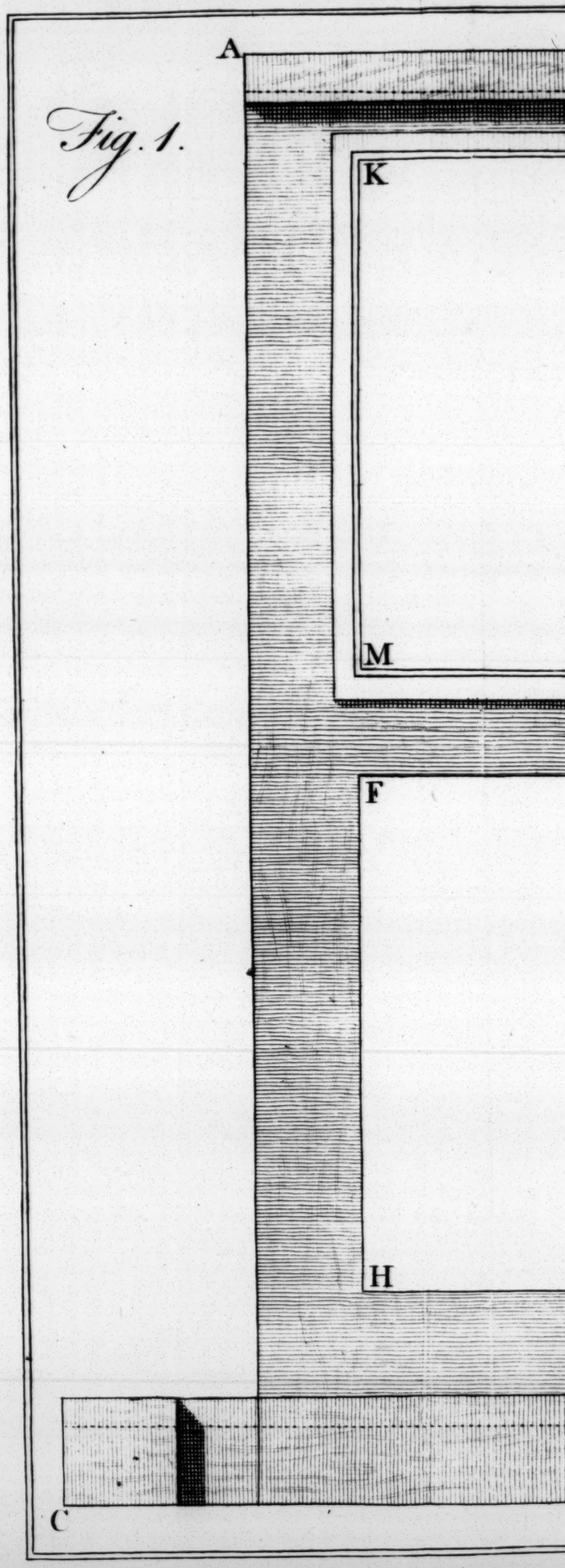
Fig. 1.



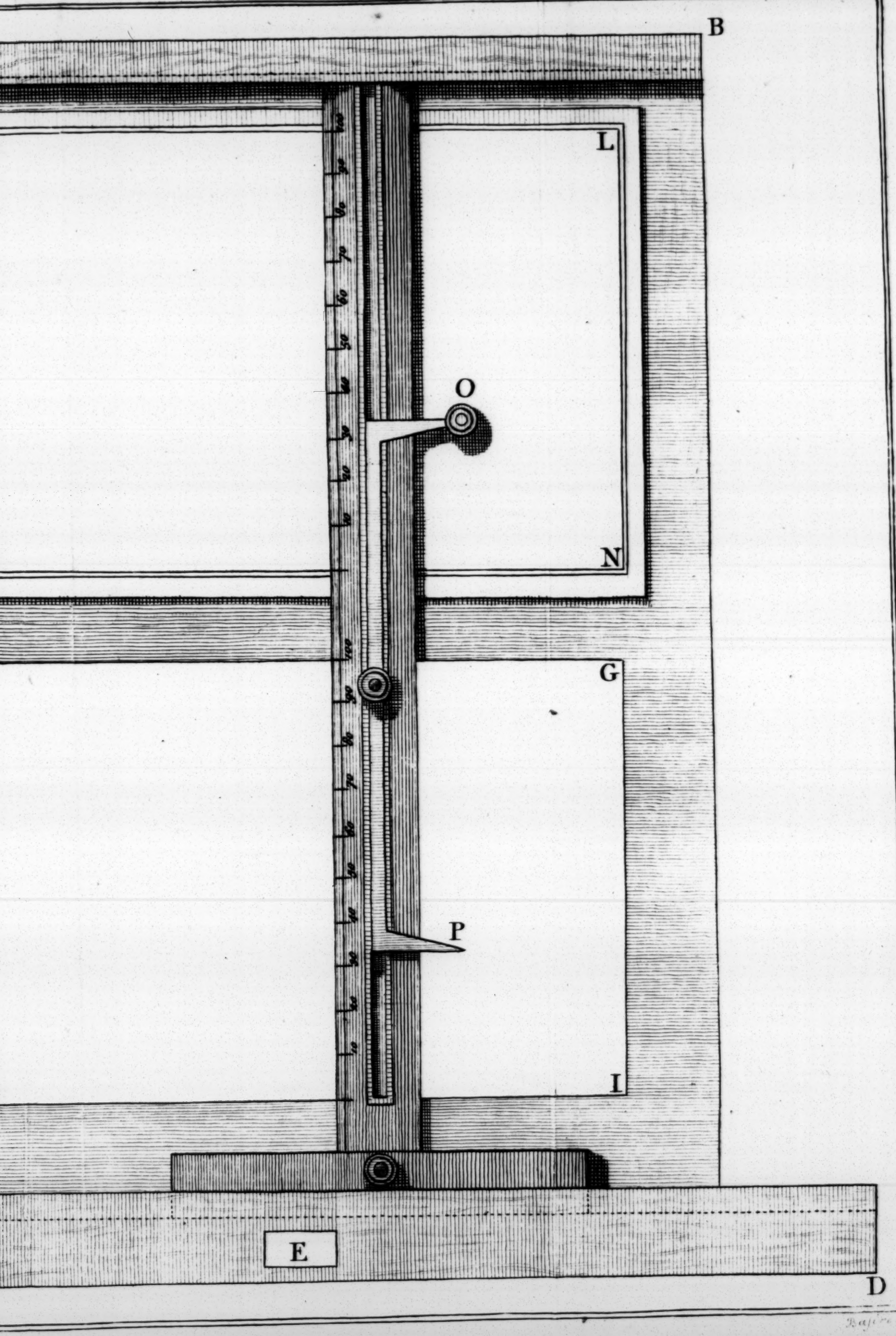

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Fig. 2.

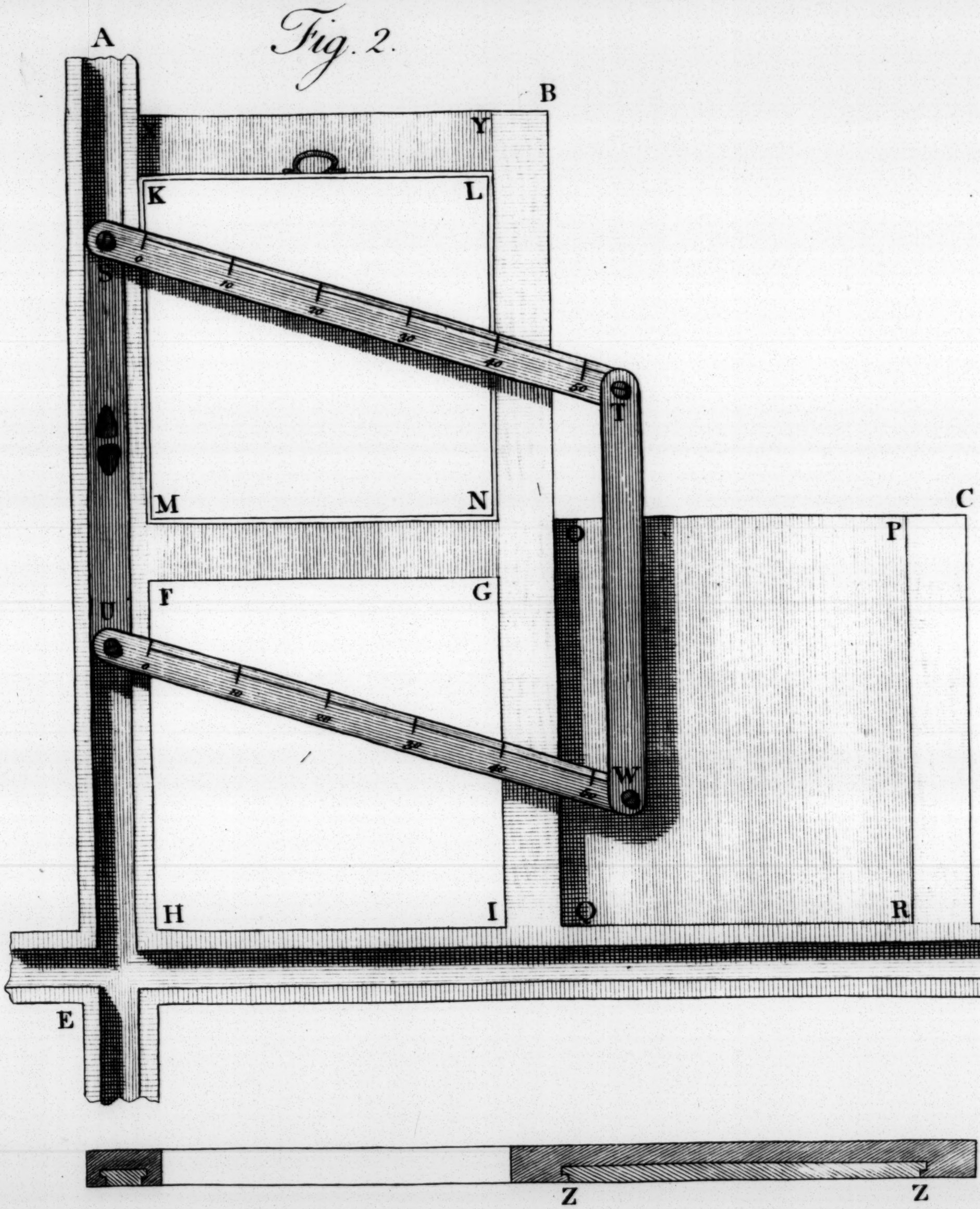
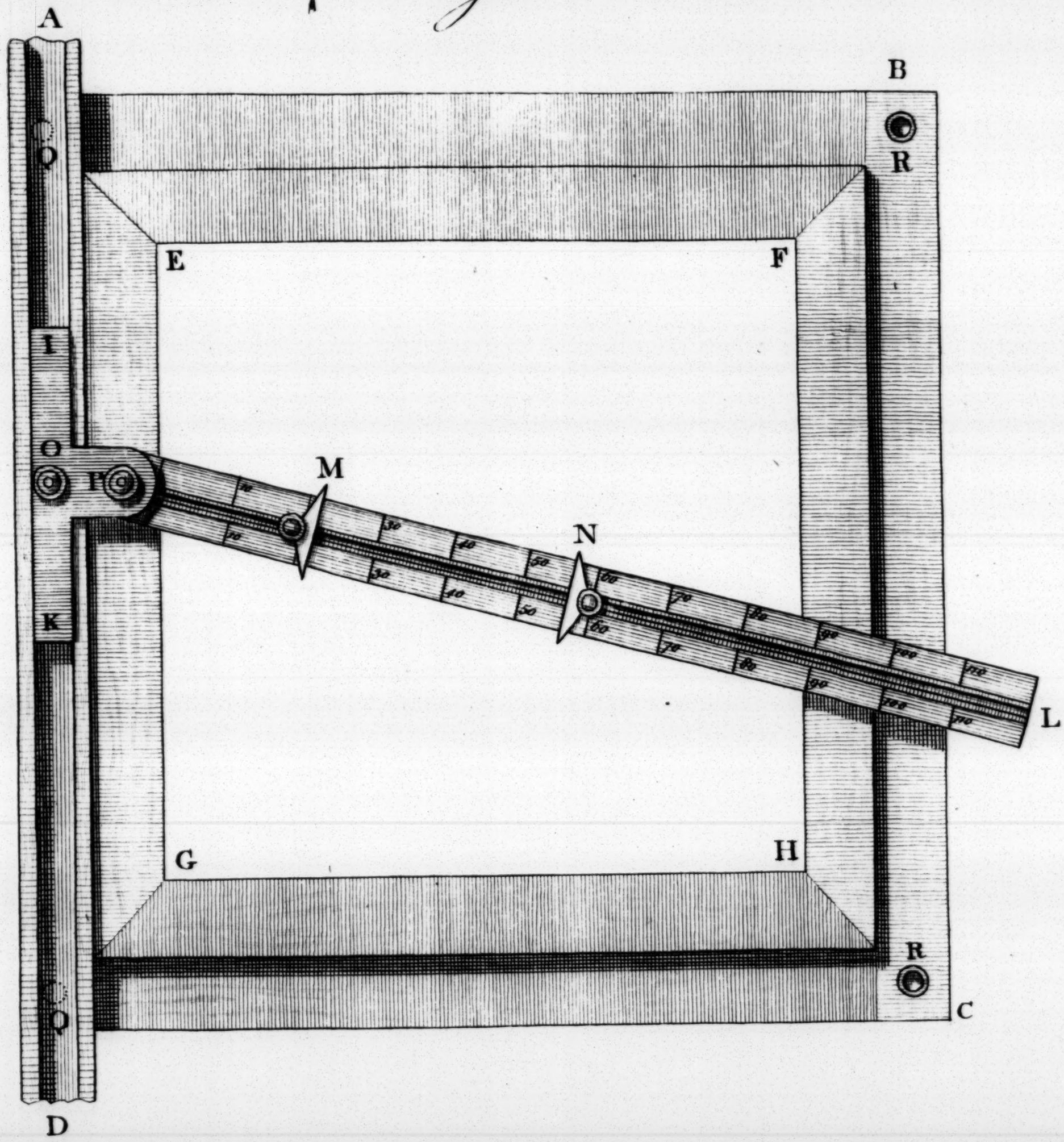


Fig. 3.



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the correſponding ſide of the board on your deſk or table, and draw the line of its preciſe length and poſition.

N. B. If this is thought too operoſe, the braſs groove and ſliding pieces M, N, may be rejected, and the blade of the ſquare may be graduated on one or both of its edges at pleaſure; and all lines in the ſame direction may be drawn thereby exactly as to their poſitions, and nearly, though ſomewhat exceeding, their lengths *; and their preciſe lengths may be determined at the ſame time the lines in the contrary poſitions are drawn, whoſe lengths will be given at the ſame time by the lines firſt drawn.

* This will be effected, by noticing the numbers upon the blade, and taking thoſe next beyond the apparent limits of the line; and by this means the drawing will advance without the leaſt confuſion.

XXIII. *Experiments on Air.*

By Henry Cavendiſh, *Eſq. F.R.S. and A.S.*

Read June 2, 1785.

IN a Paper, printed in the laſt volume of the Philoſophical Tranſactions, in which I gave my reaſons for thinking that the diminution produced in atmoſpheric air by phlogiſtication is not owing to the generation of fixed air, I ſaid it ſeemed moſt likely, that the phlogiſtication of air by the electric ſpark was owing to the burning of ſome inflammable matter in the apparatus; and that the fixed air, ſuppoſed to be produced in that proceſs, was only ſeparated from that inflammable matter by the burning. At that time, having made no experiments on the ſubject myſelf, I was obliged to form my opinion from thoſe already publiſhed; but I now find, that though I was right in ſuppoſing the phlogiſtication of the air does not proceed from phlogiſton communicated to it by the electric ſpark, and that no part of the air is converted into fixed air; yet that the real cauſe of the diminution is very different from what I ſuſpected, and depends upon the converſion of phlogiſticated air into nitrous acid.

The apparatus uſed in making the experiments was as follows. The air through which the ſpark was intended to be paſſed, was confined in a glaſs tube M, bent to an angle, as in fig. 1. (tab. XV.) which, after being filled with quickſilver, was inverted into two glaſſes of the ſame fluid, as in the figure. The air to

be

be tried was then introduced by means of a ſmall tube, ſuch as is uſed for thermometers, bent in the manner repreſented by ABC (fig. 2.) the bent end of which, after being previouſly filled with quickſilver, was introduced, as in the figure, under the glaſs DEF, inverted into water, and filled with the proper kind of air, the end C of the tube being kept ſtopped by the finger; then, on removing the finger from C, the quickſilver in the tube deſcended in the leg BC, and its place was ſupplied with air from the glaſs DEF. Having thus got the proper quantity of air into the tube ABC, it was held with the end C uppermoſt, and ſtopped with the finger; and the end A, made ſmaller for that purpoſe, being introduced into one end of the bent tube M, (fig. 1.) the air, on removing the finger from C, was forced into that tube by the preſſure of the quickſilver in the leg BC. By theſe means I was enabled to introduce the exact quantity I pleaſed of any kind of air into the tube M; and, by the ſame means, I could let up any quantity of ſoap-lees,or any other liquor which I wanted to be in contact with the air.

In one caſe, however, in which I wanted to introduce air into the tube many times in the ſame experiment, I uſed the apparatus repreſented in fig. 3. conſiſting of a tube AB of a ſmall bore, a ball C, and a tube DE of a larger bore. This apparatus was firſt filled with quickſilver; and then the ball C, and the tube AB, were filled with air, by introducing the end A under a glaſs inverted into water, which contained the proper kind of air, and drawing out the quickſilver from the leg ED by a ſyphon. After being thus furniſhed with air, the apparatus was weighed, and the end A introduced into one end of the tube M, and kept there during the experiment; the way of forcing air out of this apparatus into the tube being by

thruſting

thruſting down the tube ED a wooden cylinder of ſuch a ſize as almoſt to fill up the whole bore, and by occaſionally pouring quickſilver into the ſame tube, to ſupply the place of that puſhed into the ball C. After the experiment was finiſhed, the apparatus was weighed again, which ſhewed exactly how much air had been forced into the tube M during the whole experiment; it being equal in bulk to a quantity of quickſilver, whoſe weight was equal to the increaſe of weight of the apparatus.

The bore of the tube M uſed in moſt of the following experiments, was about one-tenth of an inch; and the length of the column of air, occupying the upper part of the tube, was in general from $1\frac{1}{2}$ to $\frac{3}{4}$ of an inch.

It is ſcarcely neceſſary to inform any one uſed to electrical experiments, that in order to force an electrical ſpark through the tube, it was neceſſary, not to make a communication between the tube and the conductor, but to place an inſulated ball at ſuch a diſtance from the conductor as to receive a ſpark from it, and to make a communication between that ball and the quickſilver in one of the glaſſes, while the quickſilver in the other glaſs communicated with the ground.

I now proceed to the experiments.

When the electric ſpark was made to paſs through common air, included between ſhort columns of a ſolution of litmus, the ſolution acquired a red colour, and the air was diminiſhed, conformably to what was obſerved by Dr. PRIESTLEY.

When lime-water was uſed inſtead of the ſolution of litmus, and the ſpark was continued till the air could be no further diminiſhed, not the leaſt cloud could be perceived in the lime-water; but the air was reduced to two-thirds of its original bulk; which is a greater diminution than it could have ſuffered by mere phlogiſtication, as that is very little more than one-fifth of the whole.

The

The experiment was next repeated with ſome impure dephlogiſticated air. The air was very much diminiſhed, but without the leaſt cloud being produced in the lime-water. Neither was any cloud produced when fixed air was let up to it; but on the further addition of a little cauſtic volatile alkali, a brown ſediment was immediately perceived.

Hence we may conclude, that the lime-water was ſaturated by ſome acid formed during the operation; as in this caſe it is evident, that no earth could be precipitated by the fixed air alone, but that cauſtic volatile alkali, on being added, would abſorb the fixed air, and thus becoming mild, would immediately precipitate the earth; whereas, if the earth in the lime-water had not been ſaturated with an acid, it would have been precipitated by the fixed air. As to the brown colour of the ſediment, it moſt likely proceeded from ſome of the quickſilver having been diſſolved.

It muſt be obſerved, that if any fixed air, as well as acid, had been generated in theſe two experiments with the lime-water, a cloud muſt have been at firſt perceived in it, though that cloud would afterwards diſappear by the earth being re-diſſolved by the acid; for till the acid produced was ſufficient to diſſolve the whole of the earth, ſome of the remainder would be precipitated by the fixed air; ſo that we may ſafely conclude, that no fixed air was generated in the operation.

When the air is confined by ſoap-lees, the diminution proceeds rather faſter than when it is confined by lime-water; for which reaſon, as well as on account of their containing ſo much more alkaline matter in proportion to their bulk, ſoap-lees ſeemed better adapted for experiments deſigned to inveſtigate the nature of this acid, than lime-water. I accordingly made ſome experiments to determine what degree of purity the

air ſhould be of, in order to be diminiſhed moſt readily, and to the greateſt degree; and I found, that, when good dephlogiſticated air was uſed, the diminution was but ſmall; when perfectly phlogiſticated air was uſed, no ſenſible diminution took place; but when five parts of pure dephlogiſticated air were mixed with three parts of common air, almoſt the whole of the air was made to diſappear.

It muſt be conſidered, that common air conſiſts of one part of dephlogiſticated air, mixed with four of phlogiſticated; ſo that a mixture of five parts of pure dephlogiſticated air, and three of common air, is the ſame thing as a mixture of ſeven parts of dephlogiſticated air with three of phlogiſticated.

Having made theſe previous trials, I introduced into the tube a little ſoap-lees, and then let up ſome dephlogiſticated and common air, mixed in the above-mentioned proportions, which riſing to the top of the tube M, divided the ſoap-lees into its two legs. As faſt as the air was diminiſhed by the electric ſpark, I continued adding more of the ſame kind, till no further diminution took place: after which a little pure dephlogiſticated air, and after that a little common air, were added, in order to ſee whether the ceſſation of diminution was not owing to ſome imperfection in the proportion of the two kinds of air to each other; but without effect *. The ſoap-lees being then poured out of the tube, and ſeparated from the quick-

* From what follows it appears, that the reaſon why the air ceaſed to diminiſh was, that as the ſoap-lees were then become neutralized, no alkali remained to abſorb the acid formed by the operation, and in conſequence ſcarce any air was turned into acid. The ſpark, however, was not continued long enough after the apparent ceſſation of diminution, to determine with certainty, whether it was only that the diminution went on remarkably ſlower than before, or that it was almoſt come to a ſtand, and could not have been carried much further, though I had perſiſted in paſſing the ſparks.

ſilver,

ſilver, ſeemed to be perfectly neutralized, as they did not at all diſcolour paper tinged with the juice of blue flowers. Being evaporated to dryneſs, they left a ſmall quantity of ſalt, which was evidently nitre, as appeared by the manner in which paper, impregnated with a ſolution of it, burned.

For more ſatisfaction, I tried this experiment over again on a larger ſcale. About five times the former quantity of ſoap-lees were now let up into a tube of a larger bore; and a mixture of dephlogiſticated and common air, in the ſame proportions as before, being introduced by the apparatus repreſented in fig. 3. the ſpark was continued till no more air could be made to diſappear. The liquor, when poured out of the tube, ſmelled evidently of phlogiſticated nitrous acid, and being evaporated to dryneſs, yielded $1\frac{4}{10}$ gr. of ſalt, which is pretty exactly equal in weight to the nitre which that quantity of ſoap-lees would have afforded if ſaturated with nitrous acid. This ſalt was found, by the manner in which paper dipped into a ſolution of it burned, to be true nitre. It appeared, by the teſt of *terra ponderoſa ſalita*, to contain not more vitriolic acid than the ſoap-lees themſelves contained, which was exceſſively little; and there is no reaſon to think that any other acid entered into it, except the nitrous.

A circumſtance, however, occurred, which at firſt ſeemed to ſhew, that this ſalt contained ſome marine acid; namely, an evident precipitation took place when a ſolution of ſilver was added to ſome of it diſſolved in water; though the ſoap-lees uſed in its formation were perfectly free from marine acid, and though, to prevent all danger of any precipitate being formed by an exceſs of alkali in it, ſome purified nitrous acid had been added to it, previous to the addition of the ſolution of ſilver. On conſideration, however, I ſuſpected, that this precipitation might ariſe from the nitrous acid in it being phlo-

gisticated; and therefore I tried whether nitre, much phlogisticated, would precipitate silver from its solution. For this purpose I exposed some nitre to the fire, in an earthen retort, till it had yielded a good deal of dephlogisticated air; and then, having dissolved it in water, and added to it some well purified spirit of nitre till it was sensibly acid, in order to be certain that the alkali did not predominate, I dropped into it some solution of silver, which immediately made a very copious precipitate. This solution, however, being deprived of some of its phlogiston by evaporation to dryness, and exposure for a few weeks to the air, lost the property of precipitating silver from its solution; a proof that this property depended only on its phlogistication, and not on its having absorbed sea-salt from the retort, or by any other means.

Hence it is certain, that nitre, when much phlogisticated, is capable of making a precipitate with a solution of silver; and therefore there is no reason to think, that the precipitate, which our salt occasioned with a solution of silver, proceeded from any other cause than that of its being phlogisticated; especially as it appeared by the smell, both on first taking it out of the tube, and on the addition of the spirit of nitre, previous to dropping in the solution of silver, that the acid in it was much phlogisticated. This property of phlogisticated nitre is worth the attention of chemists; as otherwise they may sometimes be led into mistakes, in investigating the presence of marine acid by a solution of silver.

In the above-mentioned Paper I said, that when nitre is detonated with charcoal, the acid is converted into phlogisticated air; that is, into a substance which, as far as I could perceive, possesses all the properties of the phlogisticated air of our atmosphere; from which I concluded, that phlogisticated air is

nothing elſe than nitrous acid united to phlogiſton. According to this concluſion, phlogiſticated air ought to be reduced to nitrous acid by being deprived of its phlogiſton. But as dephlogiſticated air is only water deprived of phlogiſton, it is plain, that adding dephlogiſticated air to a body, is equivalent to depriving it of phlogiſton, and adding water to it; and therefore phlogiſticated air ought alſo to be reduced to nitrous acid, by being made to unite to, or form a chemical combination with, dephlogiſticated air; only the acid formed this way will be more dilute, than if the phlogiſticated air was ſimply deprived of phlogiſton.

This being premiſed, we may ſafely conclude, that in the preſent experiments the phlogiſticated air was enabled, by means of the electrical ſpark, to unite to, or form a chemical combination with, the dephlogiſticated air, and was thereby reduced to nitrous acid, which united to the ſoap-lees, and formed a ſolution of nitre; for in theſe experiments thoſe two airs actually diſappeared, and nitrous acid was actually formed in their room; and as, moreover, it has juſt been ſhewn, from other circumſtances, that phlogiſticated air muſt form nitrous acid, when combined with dephlogiſticated air, the above-mentioned opinion ſeems to be ſufficiently eſtabliſhed. A further confirmation of it is, that, as far as I can perceive, no diminution of air is produced when the electric ſpark is paſſed either through pure dephlogiſticated air, or through perfectly phlogiſticated air; which indicates the neceſſity of a combination of theſe two airs to produce the acid. Moreover, it was found in the laſt experiment, that the quantity of nitre procured was the ſame that the ſoap-lees would have produced if ſaturated with nitrous acid; which ſhews, that the production of the nitre was not owing to any decompoſition of the ſoap-lees.

It

It may be worth remarking, that whereas in the detonation of nitre with inflammable ſubſtances, the acid unites to phlogiſton, and forms phlogiſticated air, in theſe experiments the reverſe of this proceſs was carried on; namely, the phlogiſticated air united to the dephlogiſticated air, which is equivalent to being deprived of its phlogiſton, and was reduced to nitrous acid.

In the above-mentioned Paper I alſo gave my reaſons for thinking, that the ſmall quantity of nitrous acid, produced by the exploſion of dephlogiſticated and inflammable air, proceeded from a portion of phlogiſticated air mixed with the dephlogiſticated, which I ſuppoſed was deprived of its phlogiſton, and turned into nitrous acid, by the action of the dephlogiſticated air on it, aſſiſted by the heat of the exploſion. This opinion, as muſt appear to every one, is confirmed in a remarkable manner by the foregoing experiments; as from them it is evident, that dephlogiſticated air is able to deprive phlogiſticated air of its phlogiſton, and reduce it into acid, when aſſiſted by the electric ſpark; and therefore it is not extraordinary that it ſhould do ſo, when aſſiſted by the heat of the exploſion.

The ſoap-lees uſed in the foregoing experiments were made from ſalt of tartar, prepared without nitre; and were of ſuch a ſtrength as to yield one-tenth of their weight of nitre when ſaturated with nitrous acid. The dephlogiſticated air alſo was prepared without nitre, that uſed in the firſt experiment with the ſoap-lees being procured from the black powder formed by the agitation of quickſilver mixed with lead*, and that uſed

* This air was as pure as any that can be procured by moſt proceſſes. I propoſe giving an account of the experiment, in which it was prepared, in a future Paper.

in

in the latter from turbith mineral. In the firſt experiment, the quantity of ſoap-lees uſed was 35 meaſures, each of which was equal in bulk to one grain of quickſilver; and that of the air abſorbed was 416 ſuch meaſures of phlogiſticated air, and 914 of dephlogiſticated. In the ſecond experiment, 178 meaſures of ſoap-lees were uſed, and they abſorbed 1920 of phlogiſticated air, and 4860 of dephlogiſticated. It muſt be obſerved, however, that in both experiments ſome air remained in the tube uncondenſed, whoſe degree of purity I had no way of trying; ſo that the proportion of each ſpecies of air abſorbed is not known with much exactneſs.

As far as the experiments hitherto publiſhed extend, we ſcarcely know more of the nature of the phlogiſticated part of our atmoſphere, than that it is not diminiſhed by lime-water, cauſtic alkalies, or nitrous air; that it is unfit to ſupport fire, or maintain life in animals; and that its ſpecific gravity is not much leſs than that of common air: ſo that, though the nitrous acid, by being united to phlogiſton, is converted into air poſſeſſed of theſe properties, and conſequently, though it was reaſonable to ſuppoſe, that part at leaſt of the phlogiſticated air of the atmoſphere conſiſts of this acid united to phlogiſton, yet it might fairly be doubted whether the whole is of this kind, or whether there are not in reality many different ſubſtances confounded together by us under the name of phlogiſticated air. I therefore made an experiment to determine, whether the whole of a given portion of the phlogiſticated air of the atmoſphere could be reduced to nitrous acid, or whether there was not a part of a different nature from the reſt, which would refuſe to undergo that change. The foregoing experiments indeed in ſome meaſure decided this point, as much the greateſt part of the air let up into the tube loſt its elaſticity; yet,

yet, as ſome remained unabſorbed, it did not appear for certain whether that was of the ſame nature as the reſt or not. For this purpoſe I diminiſhed a ſimilar mixture of dephlogiſticated and common air, in the ſame manner as before, till it was reduced to a ſmall part of its original bulk. I then, in order to decompound as much as I could of the phlogiſticated air which remained in the tube, added ſome dephlogiſticated air to it, and continued the ſpark till no further diminution took place. Having by theſe means condenſed as much as I could of the phlogiſticated air, I let up ſome ſolution of liver of ſulphur to abſorb the dephlogiſticated air; after which only a ſmall bubble of air remained unabſorbed, which certainly was not more than $\frac{1}{120}$ of the bulk of the phlogiſticated air let up into the tube; ſo that if there is any part of the phlogiſticated air of our atmoſphere which differs from the reſt, and cannot be reduced to nitrous acid, we may ſafely conclude, that it is not more than $\frac{1}{120}$ part of the whole.

The foregoing experiments ſhew, that the chief cauſe of the diminution which common air, or a mixture of common and dephlogiſticated air, ſuffers by the electric ſpark, is the converſion of the air into nitrous acid; but yet it ſeemed not unlikely, that when any liquor, containing inflammable matter, was in contact with the air in the tube, ſome of this matter might be burnt by the ſpark, and thereby diminiſh the air, as I ſuppoſed in the above-mentioned Paper to be the caſe. The beſt way which occurred to me of diſcovering whether this happened or not, was to paſs the ſpark through dephlogiſticated air, included between different liquors: for then, if the diminution proceeded ſolely from the converſion of air into nitrous acid, it is plain that, when the dephlogiſticated air was perfectly pure, no diminution would take place; but when it

contained

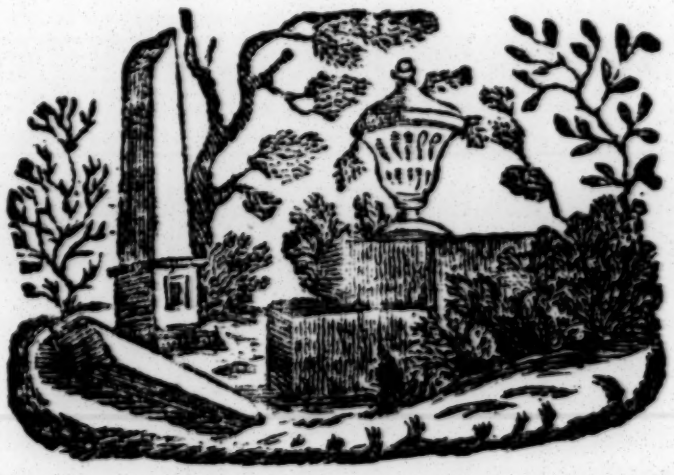
contained any phlogiſticated air, all this phlogiſticated air, joined to as much of the dephlogiſticated air as muſt unite to it in order to reduce it into acid, that is, two or three times its bulk, would diſappear, and no more; ſo that the whole diminution could not exceed three or four times the bulk of the phlogiſticated air: whereas, if the diminution proceeded from the burning of the inflammable matter, the purer the dephlogiſticated air was, the greater and quicker would be the diminution.

The reſult of the experiments was, that when dephlogiſticated air, containing only $\frac{1}{20}$ of its bulk of phlogiſticated air (that being the pureſt air I then had), was confined between ſhort columns of ſoap-lees, and the ſpark paſſed through it till no further diminution could be perceived, the air loſt $\frac{43}{200}$ of its bulk; which is not a greater diminution than might very likely proceed from the firſt-mentioned cauſe; as the dephlogiſticated air might eaſily be mixed with a little common air while introducing into the tube.

When the ſame dephlogiſticated air was confined between columns of diſtilled water, the diminution was rather greater than before, and a white powder was formed on the ſurface of the quickſilver beneath; the reaſon of which, in all probability, was, that the acid produced in the operation corroded the quickſilver, and formed the white powder; and that the nitrous air, produced by that corroſion, united to the dephlogiſticated air, and cauſed a greater diminution than would otherwiſe have taken place.

When a ſolution of litmus was uſed, inſtead of diſtilled water, the ſolution ſoon acquired a red colour, which grew paler and paler as the ſpark was continued, till at laſt it became quite colourleſs and tranſparent. The air was diminiſhed

by almoſt half, and I believe might have been ſtill further diminiſhed, had the ſpark been continued. When lime-water was let up into the tube, a cloud was formed, and the air was further diminiſhed by about one-fifth. The remaining air was good dephlogiſticated air. In this experiment, therefore, the litmus was, if not burnt, at leaſt decompounded, ſo as to loſe entirely its purple colour, and to yield fixed air; ſo that, though ſoap-lees cannot be decompounded by this proceſs, yet the ſolution of litmus can, and ſo very likely might the ſolutions of many other combuſtible ſubſtances. But there is nothing, in any of theſe experiments, which favours the opinion of the air being at all diminiſhed by means of phlogiſton communicated to it by the electric ſpark.



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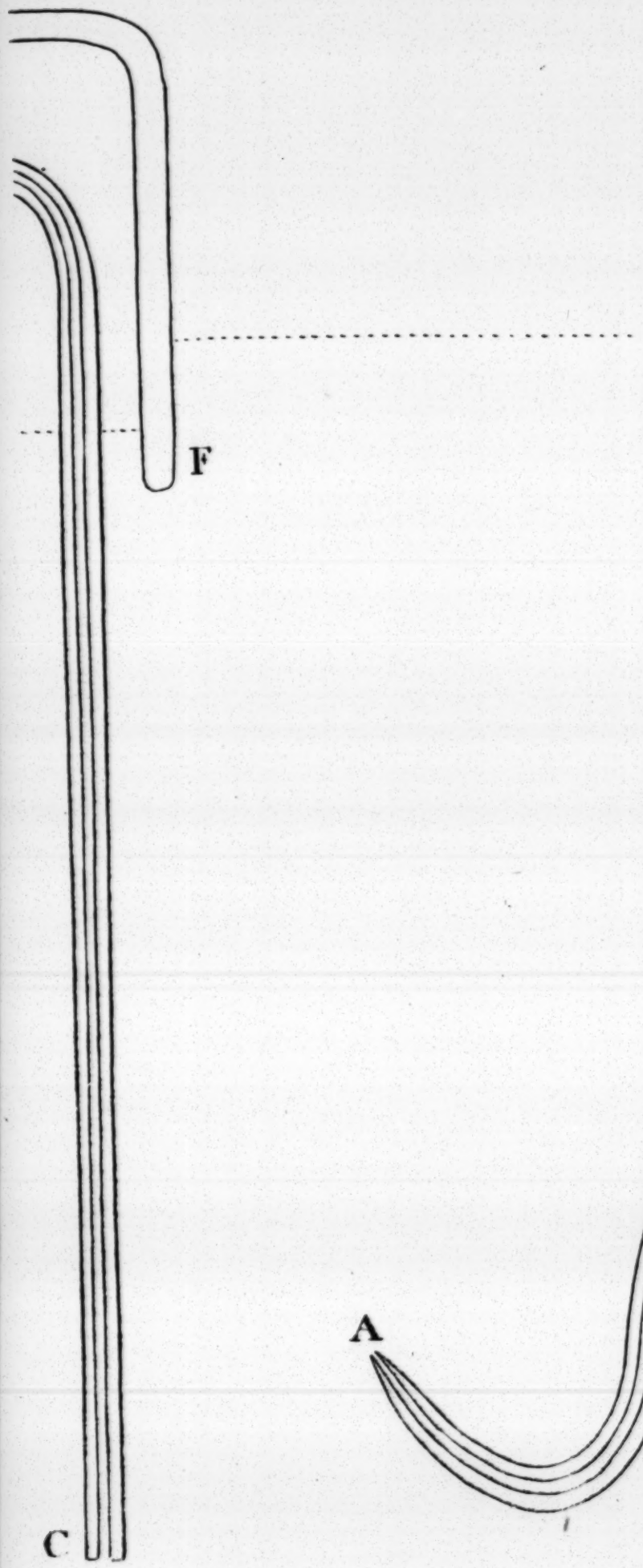

Fig. 3.

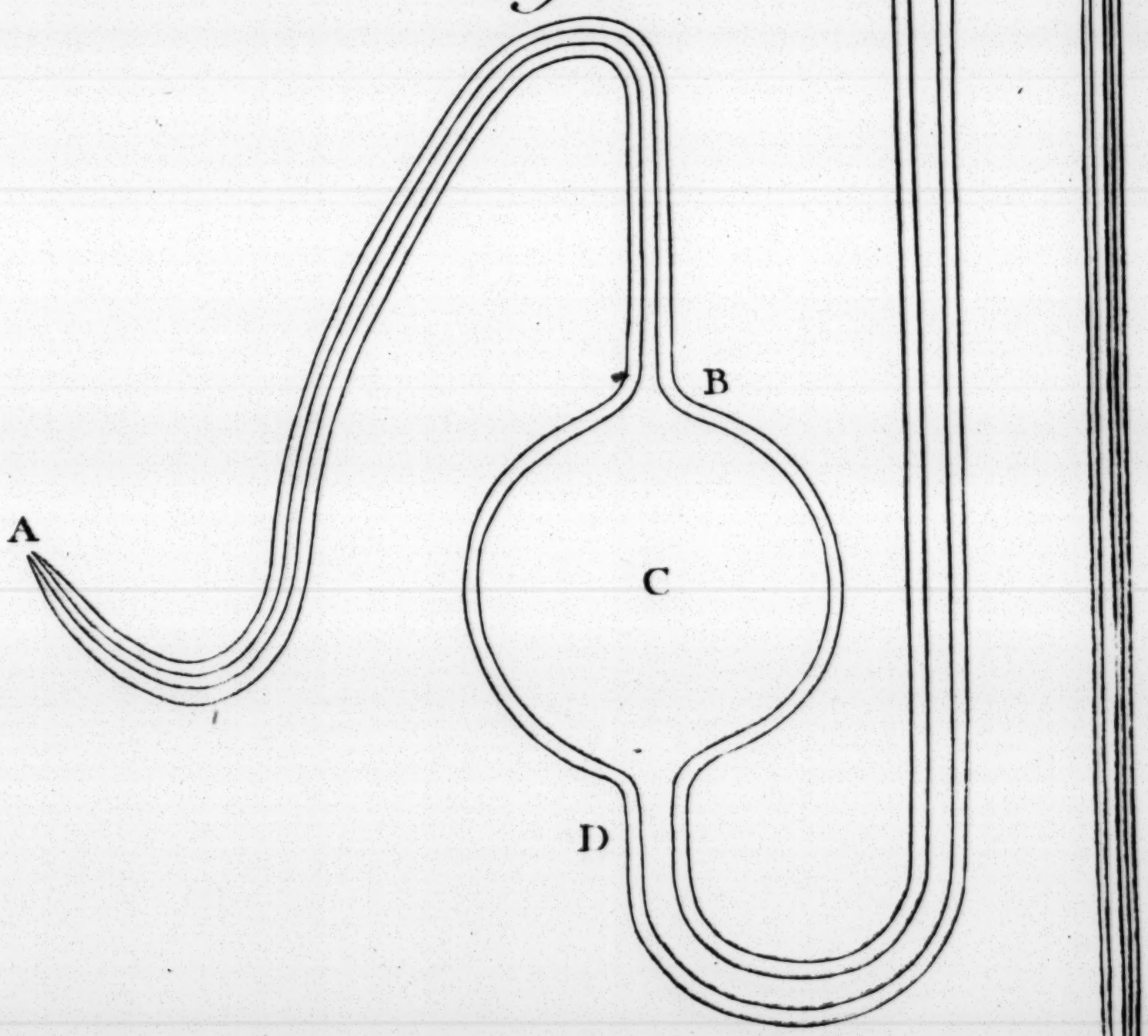

Fig. 1.

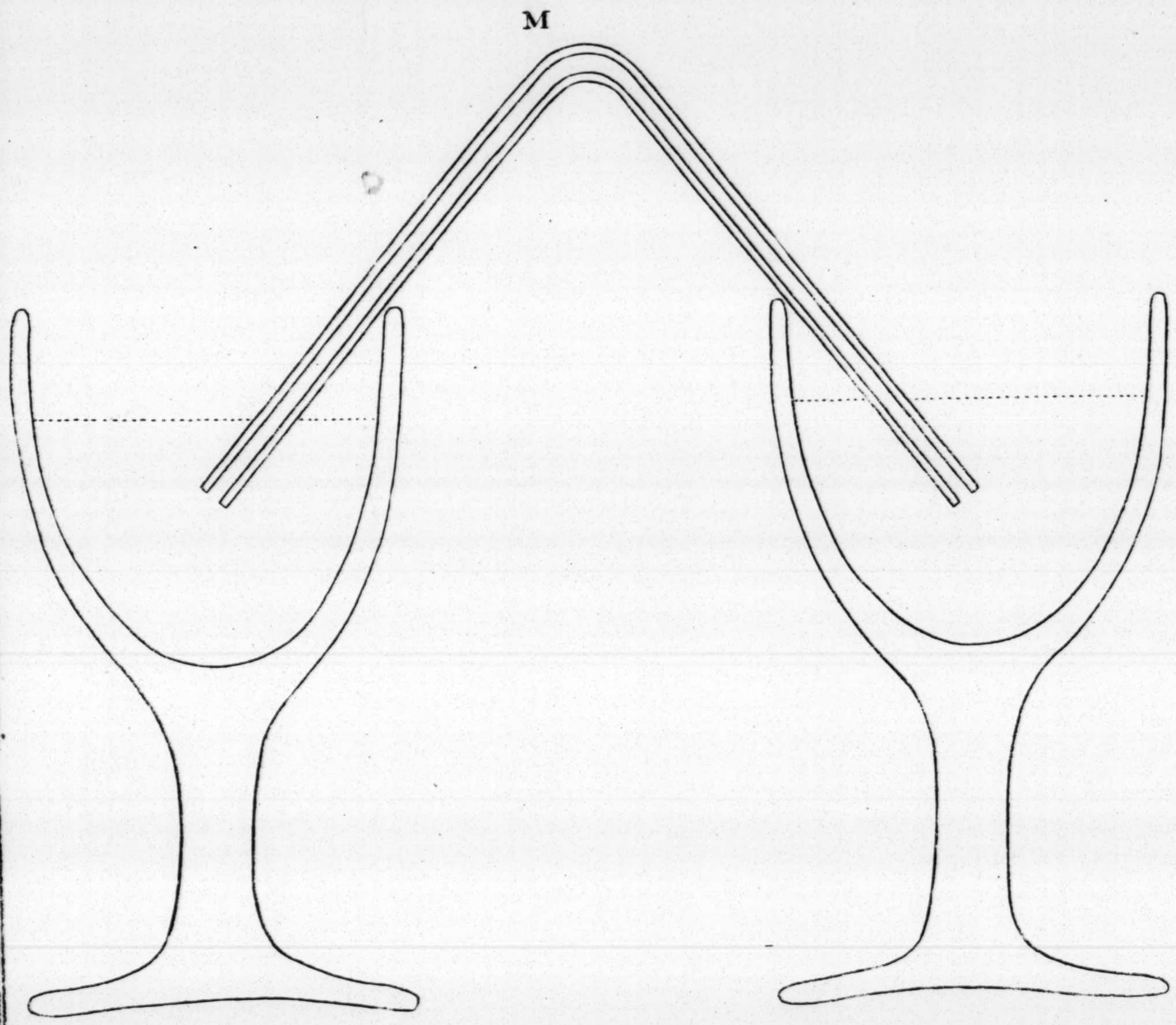

Fig. 2.

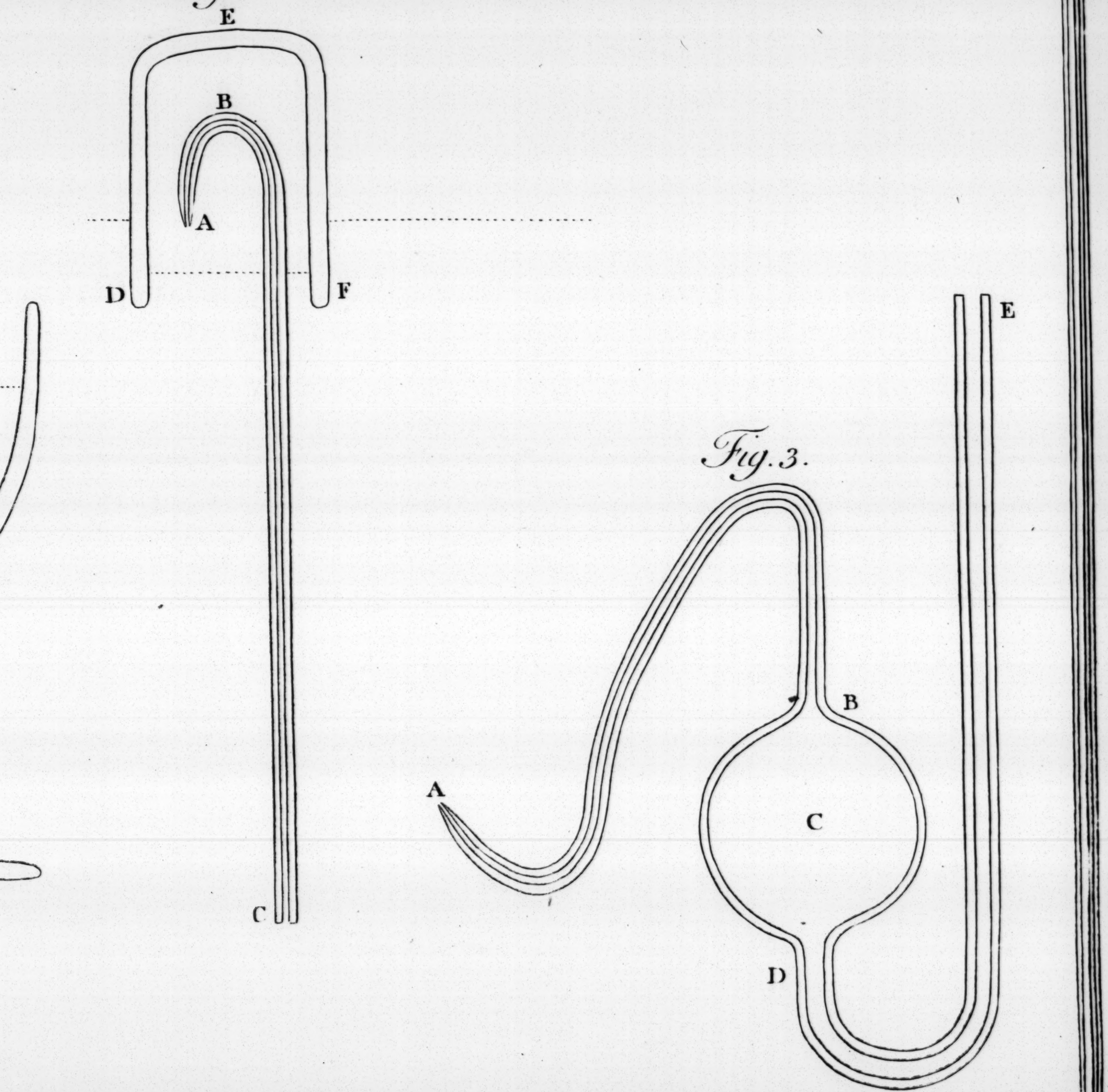

Fig. 3.

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XXIV. *An Account of the Meaſurement of a Baſe on* Hounſlow-Heath. *By Major-General* William Roy, *F.R.S. and A.S.*

Read from April 21 to June 16, 1785.

INTRODUCTION.

ACCURATE ſurveys of a country are univerſally admitted to be works of great public utility, as affording the ſureſt foundation for almoſt every kind of internal improvement in time of peace, and the beſt means of forming judicious plans of defence againſt the invaſions of an enemy in time of war, in which laſt circumſtances their importance uſually becomes the moſt apparent. Hence it happens, that if a country has not actually been ſurveyed, or is but little known, a ſtate of war generally produces the firſt improvements in its geography: for in the various movements of armies in the field, eſpecially if the theatre of war be extenſive, each individual officer has repeated opportunities of contributing, according to his ſituation, more or leſs towards its perfection; and theſe obſervations being ultimately collected, a map is ſent forth into the world, conſiderably improved indeed, but which, being ſtill defective, points out the neceſſity of ſomething more accurate being undertaken, when times and circumſtances may favour the deſign.

The riſe and progreſs of the rebellion which broke out in the Highlands of Scotland in 1745, and which was finally ſup-

preſſed, by his Royal Highneſs the late Duke of Cumberland, at the battle of Culloden in the following year, convinced Government of what infinite importance it would be to the State, that a country, ſo very inacceſſible by nature, ſhould be thoroughly explored and laid open, by eſtabliſhing military poſts in its inmoſt receſſes, and carrying roads of communication to its remoteſt parts. With a view to the commencement of arrangements of this ſort, a body of infantry was encamped at Fort Auguſtus in 1747, under the command of the late Lord BLAKENEY, at that time a Major-General; at which camp my much reſpected friend, the late Lieutenant-General WATSON, then Deputy Quarter-Maſter-General in North Britain, was officially employed. This officer, being himſelf an engineer, active and indefatigable, a zealous promoter of every uſeful undertaking, and the warm and ſteady friend of the induſtrious, firſt conceived the idea of making a map of the Highlands. As aſſiſtant Quarter-Maſter, it fell to my lot to begin, and afterwards to have a conſiderable ſhare in, the execution of that map; which being undertaken under the auſpices of the Duke of CUMBERLAND, and meant at firſt to be confined to the Highlands only, was nevertheleſs at laſt extended to the Lowlands; and thus made general in what related to the mainland of Scotland, the iſlands (excepting ſome leſſer ones near the coaſt) not having been ſurveyed.

Although this work, which is ſtill in manuſcript, and in an unfiniſhed ſtate, poſſeſſes conſiderable merit, and perfectly anſwered the purpoſe for which it was originally intended; yet, having been carried on with inſtruments of the common, or even inferior kind, and the ſum annually allowed for it being inadequate to the execution of ſo great a deſign in the beſt manner, it is rather to be conſidered as a magnificent military

ſketch, than a very accurate map of a country. It would, however, have been completed, and many of its imperfections no doubt remedied; but the breaking out of the war of 1755 prevented both, by furniſhing ſervice of other kinds for thoſe who had been employed upon it.

On the concluſion of the peace of 1763, it came for the firſt time under the conſideration of Government, to make a general ſurvey of the whole iſland at the public coſt. Towards the execution of this work, whereof the direction was to have been committed to my charge, the map of Scotland was to have been made ſubſervient, by extending the great triangles quite to the northern extremity of the iſland, and filling them in from the original map. Thus that imperfect work would have been effectually completed, and the nation would have reaped the benefit of what had been already done, at a very moderate extra-expence.

It will not be expected, that I ſhould here attempt to aſſign cauſes for the long delay that has taken place in carrying a work of ſo laudable a nature into execution: ſuffice it to ſay, that a period of twelve years having elapſed, ſince the ſcheme had been firſt propoſed, as a work that could be beſt executed in time of profound peace, without any thing being done in it, previous to the nation's being unfortunately involved in the American war; it was ſufficiently obvious, that peace muſt be once more reſtored, before any new effort could be made for that purpoſe. In the mean while, as I ſtill entertained hopes that a work which ſeemed to merit the attention of the public, would, at ſome future period, be begun, and, by gradual perſeverance, ultimately brought to perfection; therefore, in the courſe of my ordinary military employments, wherein the very beſt opportunities have offered of acquiring a thorough

thorough knowledge of the country, I have not failed to obſerve, at leaſt in a general way, ſuch ſituations as ſeemed to be the beſt adapted for the meaſurement of the baſes that would be neceſſary for the formation of the great triangles, and connecting the different ſerieſes of them together.

The peace of 1783 being concluded, and official buſineſs having detained me in or near town during the whole of that ſummer, I embraced the opportunity, for my own private amuſement, to meaſure a baſe of 7744.3 feet, acroſs the fields between the Jews-Harp, near Marybone, and Black-Lane, near Pancras; as a foundation for a ſeries of triangles, carried on at the ſame time, for determining the relative ſituations of the moſt remarkable ſteeples, and other places, in and about the Capital, with regard to each other, and the Royal Obſervatory at Greenwich. The principal object I had here in view (beſides that it might poſſibly ſerve as a hint to the public, for the revival of the now almoſt forgotten ſcheme of 1763) was, to facilitate the compariſon of the obſervations, made by the lovers of aſtronomy, within the limits of the projected ſurvey; namely, Richmond and Harrow, on the weſt; and Shooter's-Hill and Wanſted, on the eaſt: and thinking, that a Paper, containing the reſult of theſe trigonometrical operations, might not prove unacceptable to the Royal Society, I was engaged in making the computations for that purpoſe, when, very unexpectedly, I found, that an operation of the ſame nature, but much more important in its object, was really in agitation. This I ſaw would ſuperſede, at leaſt for the preſent, my own private obſervations, and perhaps render them wholly uſeleſs, unleſs it were as a matter of mere curioſity hereafter, to ſee how far ſuch as depended on a ſhort baſe, and a ſmall inſtrument (a quadrant of a foot radius) would agree with

thoſe founded on a much longer baſe, and angles determined by a large circular inſtrument, being that propoſed, as the beſt that could be made uſe of in the operation now to be mentioned.

In the beginning of October, 1783, Comte D'ADHEMAR, the French Ambaſſador, tranſmitted to Mr. Fox, then one of his Majeſty's principal Secretaries of State, a Memoir of M. CASSINI DE THURY, in which he ſets forth the great advantage that would accrue to aſtronomy, by carrying a ſeries of triangles from the neighbourhood of London to Dover, there to be connected with thoſe already executed in France, by which combined operations the relative ſituations of the two moſt famous obſervatories in Europe, Greenwich and Paris, would be more accurately aſcertained than they are at preſent *.

This Memoir the Secretary of State, by his Majeſty's command, tranſmitted to Sir JOSEPH BANKS, the very reſpectable and worthy Preſident of the Royal Society; who, about the middle of November, was pleaſed to communicate it to me, propoſing at the ſame time, that I ſhould, on the part of the Society, charge myſelf with the execution of the operation. To this propoſition I readily aſſented, on being ſoon afterwards aſſured, through the proper official channels, that my undertaking it met with his Majeſty's moſt gracious approbation.

A generous and beneficent Monarch, whoſe knowledge and love of the ſciences are ſufficiently evinced by the protection which HE conſtantly affords them, and under whoſe auſpices they are ſeen daily to flouriſh, ſoon ſupplied the funds that were judged neceſſary. What his Majeſty has been pleaſed to

* M. CASSINI's Memoir, with the Aſtronomer Royal's remarks on what is therein alledged, concerning the uncertainty of the relative ſituations of the two Obſervatories, will be given in the ſequel.

give

give ſo liberally, it is our duty to manage with proper and becoming frugality, conſiſtent with the beſt poſſible execution of the buſineſs to be done, ſo as to make it redound to the credit of the Nation in general, and of this Society in particular.

The operation, whereof we are now to give ſome account, being the firſt of the kind, on any extenſive ſcale, ever undertaken in this country, naturally enough ſub-divides itſelf into two parts. Firſt, the choice and meaſurement of the baſe, with every poſſible care and attention, as the foundation of the work; ſecondly, the diſpoſition of the triangles, whereby the baſe is to be connected with ſuch parts of the coaſt of this iſland as are neareſt to the coaſt of France, and the determination of their angles, by means of the beſt inſtrument that can be obtained for the purpoſe, from which the reſult or concluſion will be drawn. It is the firſt part only, as a ſubject of itſelf ſufficiently diſtinct, that we are now to lay before the Society; it having been judged more adviſable, to ſhew that no time has been loſt in making reaſonable progreſs, than to defer the account till the whole operation ſhould be ultimately completed.

Choice of the Baſe. Tab. XVI.

Hounſlow-Heath having always appeared to be one of the moſt eligible ſituations, for any general purpoſe of the ſort now under conſideration, becauſe of its vicinity to the Capital and Royal Obſervatory at Greenwich, its great extent, and the extraordinary levelneſs of its ſurface, without any local obſtructions whatever to render the meaſurement difficult; being likewiſe commodiouſly ſituated for any future operations of a ſimilar nature, which his Majeſty may pleaſe to order to be extended

extended from thence, in different directions, to the more remote parts of the iſland, it was propoſed to Sir JOSEPH BANKS, that the local circumſtances ſhould be actually examined; ſo far, at leaſt, as to enable us to form ſome judgement, of the beſt poſition of the line to be meaſured.

The 16th day of April, 1784, being accordingly fixed on for the purpoſe, and Mr. CAVENDISH and Dr. BLAGDEN accompanying the Preſident on this occaſion, we began our obſervations at a place called King's Arbour, at the north-weſt extremity of the Heath, between Cranford-Bridge and Longford; and having proceeded from thence through the narrow gorge, formed by Hanworth-Park and Hanworth-Farm, we finiſhed at Hampton Poor-houſe, near the ſide of Buſhy-Park, at the ſouth-eaſt extremity; the total diſtance, from the ſurvey of Middleſex, being upwards of five miles.

On this inſpection it was immediately perceived, that the firſt part of the operation, in order to facilitate the meaſurement, would be, the clearing from furze-buſhes and ant-hills, a narrow tract along the heath, as ſoon as the ground ſhould be ſufficiently dry to permit the baſe to be accurately traced out thereon.

Firſt tracing of the Baſe, and clearing of the Ground. Tab. XVI.

Chiefly with a view to the more effectual execution of the work, it was judged to be a right meaſure to obtain and employ ſoldiers, inſtead of country labourers, in tracing the baſe, clearing the ground, and aſſiſting in the ſubſequent operations. For, at the ſame time that this was obviouſly the moſt frugal method, it was evident, that ſoldiers would be more attentive to orders than country labourers; and by encamping on the

ſpot would furniſh the neceſſary centinels, particularly during the night, for guarding ſuch parts of the apparatus, as it was foreſeen muſt remain carefully untouched, in the frequent interims of diſcontinuing and reſuming the work. Accordingly, a party of the 12th regiment of foot, conſiſting of a ſerjeant, corporal, and 10 men, were ordered to march from Windſor to Hounſlow-Heath, where they encamped on the 26th of May, cloſe by Hanworth Summer-houſe, to which ſpot the neceſſary tents, camp equipage, and entrenching tools, &c. had been previouſly ſent.

Whatever might have been the particular direction given to the baſe conſidered by its extremities, from conſulting the plan it will eaſily appear, that it muſt always neceſſarily lead through the narrow gorge of the Heath formed by Hanworth-Park and Hanworth-Farm. The firſt point therefore to be attended to, in tracing it out, was, that it might lead through this paſs, without interfering with certain ponds, or gravel-pits full of water, which are in it. Theſe were eaſily avoided by carrying the line pretty near to Hanworth Summer-houſe; and in directing the teleſcope from thence towards the ſouth-eaſt, it was accidentally found, that by leaving Hampton Poor-houſe a very little to the weſtward, or right, the line would coincide with a remarkable high ſpire, ſeen at the diſtance of eleven or twelve miles, and known afterwards to be Banſted-Church. As there could not be a better ſituated, or more conſpicuous object than this, therefore the firſt or ſouth-eaſt ſection of the baſe, comprehended between the Summer-houſe and the angle of the ſmall field adjoining to Hampton Poor-houſe, was immediately directed upon it; and the ſoldiers were the ſame day ſet to work to clear the tract, which, at a medium, was made from two to three yards in breadth. This

operation

operation continued eight or ten days, owing to the lower part of the heath, between Wolſey-River and the Poor-houſe, being encumbered with bruſh-wood.

When the clearing of the firſt ſection was completed, the ſecond, comprehended between the Summer-houſe and the great road leading from Staines to London, was traced out in the following manner. One of the pyramidal bell-tents (whereof two had been provided, one of twenty-five, and the other of fifteen feet in height) being placed at the ſtation near the Summer-houſe, camp colours were then arranged from diſtance to diſtance, ſo as to be in a line with the bell-tent and Banſted ſpire. In like manner, the third ſection, comprehended between the Staines Road and King's Arbour, was traced out.

This firſt tracing of the baſe was done by means of a common teleſcope held in the hand only, that no time might be loſt in employing the ſoldiers to ſmooth the tract which was to be meaſured; becauſe the tranſit inſtrument (my own property, for which a portable ſtand had been for ſome time preparing) was not yet ready to be applied, as it afterwards was, in tracing out the baſe more accurately.

The camp ſtill remained, where it was originally pitched, at the angle of Hanworth-Park, this being a very convenient poſition, with regard to the firſt and ſecond ſections; but being too remote from the third, that time might not be loſt, and the men unneceſſarily fatigued in marching backwards and forwards; therefore, one half of the party, under the command of the corporal, was detached to the northward, and quartered in the neighbouring villages, to clear the third ſection, while the ſerjeant, with the remainder, were occupied in ſmoothing the ſecond. Owing to the extraordinary wetneſs of the ſeaſon, this operation required more time than had been at firſt

imagined, not having been entirely finiſhed before the firſt week of July. We ſhall therefore leave it going on, and in the mean time proceed to deſcribe the inſtruments that were ſubſequently made uſe of in the firſt and ſecond meaſurements.

Steel Chain. Tab. XVII.

One of the firſt inſtruments, which that able artiſt Mr. RAMSDEN had orders to prepare, was a ſteel chain, one hundred feet in length, the beſt that he could make. Not that it was intended, nor could it be ſuppoſed, that we ſhould abſolutely abide by the reſult that this chain ſhould furniſh us with, for the length of the baſe; but it was hoped, that an inſtrument of this ſort might be made, which would meaſure diſtances much more accurately than any thing of that kind had ever done before: and it was conſidered as an object of ſome conſequence, to endeavour to ſimplify, and render as eaſy as poſſible, the meaſurement of baſes in future: an operation which, hitherto, has always been found to be tedious and troubleſome, to which we may now further add, uncertain likewiſe, when done with rods of deal, as will appear from the account hereafter to be given.

The conſtruction of the chain, which is on the principles of that of a watch, will be underſtood from the repreſentation of ſome of its chief parts, to the full ſize, in tab. XVII. where the firſt, or zero-end link, is ſhewn both in plan and elevation, in the ſtate in which it was originally applied to meaſurement on the ſurface of the ground. Each link conſiſts of three principal parts; namely, a long plate; two ſhort ones, half the thickneſs of the former, with circular holes near the

extremities of each; and two caſt-ſteel pins, or axes, ſuited to the diameters of the holes, which ſerve to connect the adjoining links together. The holes in the ſhort plates are made rough or jagged with a file; ſo that when they have embraced the ends of two adjoining long ones, and the pins have paſſed through all the holes, in rivetting their extremities, they are made perfectly faſt, and as it were united to the ſhort plates; while the embraced ends of the long ones turn freely round on the middle part of the pins.

At every tenth link the joint, juſt now deſcribed, has a poſition at right-angles to the former; that is to ſay, the ſhort plates lie here horizontally, and the pins paſſing through them ſtand vertically. Thus, there being in the whole chain two hundred caſt-ſteel pins, one hundred and eighty lie horizontally; and twenty, including the two by which the handles are attached, ſtand vertically. Theſe croſs-joints, which were chiefly intended that the chain might fold up in a ſmaller compaſs, by returning upon itſelf at every tenth link, are likewiſe uſeful in preſenting a horizontal ſurface, to which ſmall circular pieces of braſs are ſcrewed, with figures 1, 2, 3, &c. to 9, engraved on them, denoting the decimal parts of the length. Thus the middle croſs-joint, or that which ſeparates the 50th from the 51ſt link, is ſhewn in the Plate with the figure 5 upon it.

The chain, in its firſt conſtruction (for we are now to point out ſome alterations that were afterwards made in it), was one hundred feet in length, including the two braſs handles; in the extremity of each of which there was a ſemi-circular hole, of the ſame diameter with the ſteel arrows ſucceſſively fixed in the ground, and ſerving to keep the account of the number of chains, when applied to common meaſurement. In this its

firſt

firſt mode of application it was ſoon diſcovered, as we ſhall have occaſion to mention hereafter, how admirably the chain performed; and that, with ſome farther precautions, a ſtill greater degree of exactneſs might be attained, by ſupporting it on ſtands, or even on planks, laid on, or but little removed from, the common ſurface of the earth. For this purpoſe, the two end-links were altered, each being now made equal to one foot, excluſive of the handles. By referring to tab. XVII. the nature of this alteration will be eaſily conceived. It conſiſted in ſcrewing to the under ſide of the handles, very near the joints, two feather-edged pieces of braſs *; the one denoting zero, and the other 100 feet. Over the dart at the firſt, a plummet with a fine ſilver wire being ſuſpended, that wire, by a very ſimple apparatus, hereafter to be deſcribed, may be brought accurately to coincide with any point whatever of commencement: and at the ſecond, a fine line with a knife, or other ſharp inſtrument, being drawn on a piece of card placed there for the purpoſe, and changed as often as needful; or, as was likewiſe practiſed, and found to anſwer better, a line on a moveable ſlide of braſs, attached to the top of the ſtand or plank, being brought to coincide with the feather-edge, and then faſtened underneath; the extremity of the 100 feet is readily aſcertained: and thus the meaſurement may be continued on with great accuracy to any diſtance at pleaſure.

That the chain, in this its altered ſtate, may ſtill be advantageouſly applied to ordinary meaſurement on the ſurface of the earth, the pieces above deſcribed, having ſteady pins, and being faſtened with ſcrews, can be eaſily removed, and others,

* They were originally of braſs, but are now of ſteel, that the edges by being harder might run leſs riſk of being damaged.

exactly of the ſame length, ſubſtituted in their ſtead, with ſemi-circular holes (as repreſented in the Plate by dotted lines near the joint of the handle) to receive the ſteel arrows, then to be made uſe of in the manner already mentioned.

This moſt excellent chain ſeems not to have ſuffered any perceptible extenſion from the uſe that has hitherto been made of it. It is ſo accurately conſtructed, that when ſtretched out on the ground, as in common uſe, all the long plates lying vertically or edge-wiſe, if a perſon, laying hold of either end with both hands, gives it a flip or jerk, the motion is, in a few ſeconds, communicated to the other end, in a beautiful vertical ſerpentine line; when the perſon, holding that handle, receives a ſudden ſhock, by the weight of the chain pulling him forcibly. The chain weighs about eighteen pounds, and when folded up is eaſily contained in a deal box, about fourteen inches long, eight inches broad, and the ſame in depth.

Deal Rods. Tab. XVIII.

The baſes which have hitherto been meaſured in different countries, with the greateſt appearance of care and exactneſs, have all, or for the moſt part, been done with deal rods of one kind or other, whoſe lengths being originally aſcertained by means of ſome metal ſtandard, were, in the ſubſequent applications of them, corrected by the ſame ſtandard. Having thus had ſo many precedents, ſerving as examples to guide us in our choice, it was natural enough that we ſhould purſue the ſame method in the meaſurement to be executed on Hounſlow-Heath; taking, however, all imaginable care, that our rods ſhould be made of the very beſt materials that could be procured;

cured; with this farther precaution, that by truſſing them, they ſhould be rendered perfectly inflexible, a circumſtance not before attended to.

As ſome difficulty had been found in procuring well-ſeaſoned Pine-wood of ſufficient length, and perfectly free from knots, for the intended purpoſe; therefore Sir JOSEPH BANKS had early applied to the Admiralty for aſſiſtance in this reſpect; and forthwith obtained an order to be furniſhed with what we might have occaſion for, from his Majeſty's yard at Deptford, where an old New-England maſt, and alſo one of Riga wood, were ſpeedily cut up for our uſe.

New-England white Pine is lighter, leſs liable to warp, and leſs affected by moiſture, than Riga red wood. But the New-England maſt, when it came to be very minutely examined, was found to be too much wounded by ſhot-holes in ſome parts, or too much decayed or knotty in others, to afford us a ſufficiency. This being the caſe we had recourſe to the Riga wood, which was indeed extremely ſmooth and beautiful; and ſo perfectly ſtraight-grained, that a fibre of it, when lifted up, might be drawn, like a thread, almoſt from one end to the other.

It had been in contemplation, to make the rods of twenty-five or thirty feet in length; and one of the former dimenſions was actually conſtructed: but this being found to be rather too unwieldy, it was judged beſt to content ourſelves with thoſe of about twenty feet.

Different opinions have been entertained with regard to the beſt mode of applying rods in meaſurement; ſome contending that contacts, or that of butting the end of one rod againſt the end of the other, is the beſt; while others (with more probability of being

being right) are of opinion, that the adjuſtment by the coincidences of lines ſhould have the preference. The firſt is undoubtedly the moſt expeditious method; but ſeems at the ſame time to be liable to this very objectionable circumſtance, that the probable errors fall all one way: whereas, in the ſecond method, although by far the moſt tedious, the errors of coincidence falling ſometimes on one ſide, and ſometimes on the other, they compenſate for, or deſtroy, each other; and therefore no error is committed.

With the view of ſatisfying both parties, and in order to put the matter, if poſſible, out of doubt, it was judged proper to conſtruct the rods in ſuch a manner as to admit of both methods being tried, that we might adhere to that which ſhould be found by experience to be the beſt.

Three meaſuring rods were accordingly ordered to be made, and alſo a ſtandard rod, with which the former were from time to time to be compared. Their general conſtruction will be better conceived from the plan and elevation, and other repreſentations of their principal parts, in tab. XVIII. than by any deſcription, however particular, conveyed in words. It will be ſufficient to ſay, that the ſtems of the three meaſuring rods are each twenty feet three inches in length, reckoning from the extremities of the bell-metal tippings; very near two inches deep; and about 1 $\frac{1}{4}$ inch broad. Being truſſed laterally and vertically, they are thereby rendered perfectly, or at leaſt as to ſenſe, inflexible. The ſtandard rod could only be truſſed laterally; and it is juſtly repreſented by the plan of the other rods, excepting that its ſtem is ſomething ſtronger, and that it has two or three inches at each end of extra-length, the reaſons for which differences will appear hereafter.

By referring to the Plate it will be obſerved, that two narrow pieces of ivory, each faſtened with two ſmall ſcrews, are inlaid into the upper ſurface of the rods, within one inch and a half of the extremities of the tippings. Theſe ivory pieces received the fine black lines cut into them when the lengths of the rods were laid off, in the manner hereafter to be mentioned, and accurately determined the intermediate diſtance of 20 feet, or 240 inches, the meaſure to be uſed in the application by coincidences: whereas, in that by contacts, the ſpace comprehended between the extremities of the projecting lips of the tippings, is 243 inches.

Immediately behind each ivory piece, a cavity is formed underneath, in the middle of the ſtem. This receives a braſs wheel, about eight-tenths of an inch in diameter, whoſe axis turns in the fork of a braſs ſpring, five inches long, faſtened by a ſcrew to the under ſurface juſt before the croſs feet. Theſe ſprings are only of ſuch ſtrength as to permit the wheels to be forced up into the cavities by the weight of the rod, which, in its adjuſted ſtate, always reſts entirely on the ſurfaces of the two ſtands that ſupport its extremities. But when the rod is to be raiſed from the ſtands, then the milled-headed ſcrews, projecting above the upper ſurface, and ſtanding over the middle of the ſprings, being brought to act, the wheels are thereby preſſed downwards, and receive the full weight of the rod, which is then eaſily moved backwards or forwards to its true poſition, either of contact or coincidence.

The croſs-feet, placed about $5\frac{1}{2}$ inches from the ends of the rods, and $1\frac{3}{4}$ inch from the inſertion of the truſſings, are each about nine inches long, $1\frac{1}{2}$ broad, and nearly an inch in depth, having their lower ſurfaces level with that of the ſtem. By

means of theſe, the rods are not only kept more ſteady on the ſtands, againſt the common action of the wind upon the truſſings; but they likewiſe ſerve as holds for the vertical and horizontal braſs clamps, whereby the rods are made faſt to the ſtands on one ſide or other, and in both modes of application, contacts and coincidences; as will be more fully explained hereafter, in deſcribing the tops of the ſtands.

Braſs Standard Scale, and method of laying off the lengths of the Deal Rods.

At the ſale of the inſtruments of the late ingenious optician Mr. JAMES SHORT, I purchaſed a finely divided braſs ſcale, of the length of 42 inches, with a VERNIER's diviſion of 100 at one end, and one of 50 at the other, whereby the 1000th part of an inch is very perceptible. It was originally the property of the late Mr. GRAHAM, the celebrated Watch-maker; has the name of JONATHAN SISSON engraved upon it; but is known to have been divided by the late Mr. BIRD, who then worked with SISSON.

It is ſufficiently well known to this Society, that their braſs ſtandard ſcale, about 42 inches long, which contains on it the length of the ſtandard yard from the Tower, that from the Exchequer, and alſo the French half-toiſe, together with the duplicate of the ſaid ſcale, ſent to Paris for the uſe of the Royal Academy of Sciences, were both made by Mr. JONATHAN SISSON, under Mr. GRAHAM's immediate direction. Now, although there ſeemed to be every reaſon to ſuppoſe, that the ſcale at preſent in my poſſeſſion, originally Mr. GRA-

HAM's property, would correſpond with thoſe above-mentioned, which he had been directed by the Royal Society, with ſo much care and pains, to provide; yet, that nothing of this ſort might remain doubtful, it was judged right, in ſettling the abſolute length of the baſe, which I meaſured near London in 1783, as has been mentioned in the introduction to this Paper, that the two ſcales ſhould be actually compared. Having accordingly obtained an order from the Preſident, for admiſſion into the Society's Apartments, I went there in the afternoon of the 13th of Auguſt, and laid both ſcales taken out of their caſes on the table of the meeting-room, with thermometers along-ſide of them, that they might acquire the ſame temperature. On the forenoon of the 15th of Auguſt the compariſon was made, with the aſſiſtance of Mr. RAMSDEN, who for that purpoſe carried along with him his curious beam-compaſſes, whoſe micrometer-ſcrew ſhews very perceptibly a motion of $\frac{1}{5000}$th part of an inch. Thus the extent of three feet, being carefully taken from the Society's ſtandard, and applied to my ſcale, it was found to reach exactly to 36 inches, the temperature being 65°. In like manner, the beam-compaſſes being applied to the length of the Exchequer yard, the extent was now found by the micrometer to overreach that yard by $\frac{69}{10000}$th, or nearly $\frac{7}{1000}$th parts of an inch.

Having thus ſhewn that my ſcale is accurately of the ſame length with the Society's ſtandard, it remains to point out the uſe that was made of it, for aſcertaining the lengths of the deal rods, intended for the operation on Hounſlow-Heath. In the firſt place, Mr. RAMSDEN prepared a beam-compaſs, ſufficient to take in twenty feet, truſſed in all reſpects like the

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meaſuring rods, but ſomething deeper, and fitted as uſual with proper points and micrometer. The ſtandard rod being now conſtructed was laid on the ſhop-board, ſtrongly framed for the purpoſe, and nearly level. To one ſide of it, at the diſtance of about twenty feet two inches from center to center, two ſtrong bell-metal cocks were firmly ſcrewed. Theſe cocks were about 2¼ inches in length, three-eighths in thickneſs, and roſe above the ſtem nearly two inches, ſo as to be on the ſame plane with the ſurface of the meaſuring rods, when placed upon it.

A large plank, cut from the New-England maſt, upwards of thirty feet long, nine or ten inches broad, and about three inches thick, being ſet edge-wiſe in the ſame room, on part of the ſtands now ready for the operation, was, in that poſition, planed perfectly ſmooth and ſtraight. A ſilver wire being then ſtretched very tight, along the middle of the plank, from one end to the other, ſix ſpaces of forty inches each were marked off by the ſide of the wire, at which points ſeven braſs pins, about one-tenth of an inch in diameter, were driven into the wood, and their tops poliſhed with the ſtone. During the whole of this operation, and that which followed, the thermometer, lying by the ſide of the braſs ſcale, continued ſteadily at or very near 63°.

A fine dot being now made on one of the extreme pins, and the ſilver wire being ſtretched over the dot, and as near as poſſible over the middle of the other pins, in which poſition it was made faſt; the extent of forty inches, taken with the utmoſt care from the braſs ſcale, was then marked off, by placing one point of the beam-compaſſes in the dot, and with the other deſcribing a ſhort faint arc on the ſurface of the ſe-

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cond pin. The beam being then removed, and one point placed in the interſection of the arc and wire, with the other point a dot was made on the third pin, under the middle of the wire. Upon this dot, as a center, a faint arc was next deſcribed on the ſame pin where the firſt had been traced. In this manner the ſix times forty inches were marked off, alternately with dots and arcs; a method found by Mr. RAMSDEN, in his practice, to be more accurate, than when dots only are made uſe of.

The exact length of twenty feet, thus obtained, was next taken between the points of the long beam-compaſſes, and transferred to the tops of the bell-metal cocks, placed, as has been already mentioned, on the ſide of the ſtandard rod, in ſuch manner as to leave more than one inch and a half of the ſaid cocks beyond or without the lines denoting the extent of the twenty feet. This being done, the meaſuring rods were ſucceſſively placed on the ſtandard, and their ſides applying cloſe to the cocks, the diſtance of twenty feet was readily transferred from them to the inlaid ivory pieces, on which fine lines were afterwards cut, by marks accurately made for that purpoſe.

With regard to the adjuſtment of the lips of the bell-metal tippings, which extend exactly one inch and a half beyond the ivory lines, ſo as to make the total length of the rod 243 inches, it is to be obſerved, that they terminate in flat curves of $3\frac{1}{2}$ inches radius, paſſing through the inch and half points, to which they were cautiouſly ground down, that at firſt they might rather exceed than be defective in length. Any two of the rods, lying in the ſame plane, and alſo in the ſame ſtraight line, being brought into contact with each other; if

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of the true length, the ſpace in that poſition, comprehended between the two lines on the inlaid ivory pieces, muſt be exactly three inches. For the purpoſe of this adjuſtment, the extent of three inches was therefore taken from the braſs ſcale and cut upon the ſide of a detached piece of ivory; which being readily applied to the aforeſaid intermediate ſpace, the ſame was gradually reduced, by grinding the lips equally, till it exactly correſponded with that taken from the ſcale.

The three rods are numbered by a cypher on the ſurface of the metal at each end, 1.2; 3.4; 5.6; and that being the order in which they were to be applied in actual meaſurement, ſo it was likewiſe the order in which they were adjuſted; that is to ſay, the rod 1.2 was adjuſted with 3.4, and with 5.6; and the rod 3.4 was, in like manner, adjuſted with 1.2 and 5.6.

One of theſe deal rods, when finiſhed, was found to weigh twenty-four pounds. They were intended to be contained in two cheſts, one large and the other ſmaller. The large cheſt, which is about 2½ feet deep, may be called a double one, becauſe it has two lids that lift quite off, which, in turning upſide down, become alternately top and bottom, having between them, but much nearer to the one than the other, a bottom that is common to both. The ſhallow ſide holds the ſtandard rod; and the other, two of the meaſuring rods; which laſt is rendered practicable by having one of the ſide braces of each fixed only with ſcrews, ſo as to be removed and replaced at pleaſure. Thus one of the rods being laid in its place, the other is put over it in an inverted poſition; and both having the proper faſtenings to keep them in their poſitions, the lid is then put on, and fixed by ſcrews. The cheſt being now turned upſide down, and the other lid removed, the ſtandard is thereby

thereby diſcovered reſting on the common bottom, which has bands laid acroſs it for the purpoſe, a few inches below what has now become the ſurface of the cheſt. It was neceſſary that the ſtandard ſhould reſt thus high, both that the light might come freely upon it, and that, being ſupported by the deep ſides of the cheſt, it might be prevented from twiſting, for it will be remembered that it is only truſſed laterally. By means of a ſmall braſs ſpring fixed to each end of the ſtandard, a fine ſilk thread, as being leſs liable to accident than ſilver wire, is ſtretched along its ſtem, which by ſmall wedges prepared for the purpoſe, and ſlipped in between it and the bands on which it reſts, is always brought into the ſame poſition. This being done, the ſilk thread is turned off, ſo as to permit the meaſuring rods to be laid on the ſtandard for compariſon. With regard to the ſmaller cheſt, ſuch a one was actually made, and ſent down to the heath, towards the cloſe of the operation with the deal rods; but from ſome miſtake in its dimenſions, it would not admit the third rod.

Stands for the Meaſuring Rods. Tab. XVIII. and XIX.

From the extraordinary levelneſs of Hounſlow-Heath, the aſcent from the ſouth-eaſt towards the north-weſt being little more than one foot in a thouſand in the diſtance of five miles, it was eaſily ſeen, that the computed baſe-line, or that actually forming a curve parallel to the ſurface of the ſea, at that height above it, would fall ſo little ſhort of the hypothenuſal diſtance, meaſured on, or parallel to, the ſurface of the Heath, as ſcarcely to deſerve notice, had it not been thought neceſſary to

ſhew, how much one end of the baſe was really higher than the other; and to convince the world, that in an operation of this ſort, where ſo much accuracy was expected, no pains were ſpared, nor the moſt trivial circumſtances neglected.

From the trouble and uncertainty attending the frequent uſe of plummets, eſpecially in windy weather, inſtead of meaſuring level or baſe lines, as has hitherto been cuſtomary (in which caſe it would have been neceſſary to make uſe of the plummet, or ſome ſuch contrivance, at every ſtep of aſcent or deſcent) it was judged to be a better method to meaſure hypothenuſes, and, having obtained the relative heights of the ſtations by the accurate application of the teleſcopic ſpirit-level, to compute the baſe lines. Thus it was propoſed, that the length of the baſe on Hounſlow-Heath ſhould be obtained by meaſuring a line through the air, drawn parallel to the common ſurface from ſtation to ſtation, in equal diſtances of 200 yards or 600 feet each, as repreſented in the figure at the top of tab. XVIII.

For this purpoſe, two kinds of ſtands were uſed; one whoſe height was fixed, to be placed at the beginning and end of each 200 yards; and the others, whoſe heights were moveable, that their ſurfaces might be brought more eaſily to coincide with the line paſſing through the air from one fixed ſtand to the other. The fixed ſtands in their firſt ſtate, repreſented by that towards the left-hand in the plate for the deal rods, were only two feet ſeven inches in height; but when the glaſs rods were afterwards uſed, they had an additional piece of ten inches faſtened to the top (as in the left-hand ſtand of tab. XIX.) which made their total height above the Heath, including the platform on which they ſtood, three feet and a half. They are tripods of white deal, whoſe legs extend about

three feet from each other; and being braced diagonally, are mortoiſed at top into circles of the ſame ſort of wood. Over this circle, a ſquare table of about 11½ inches is fixed, compoſed of oak, and mahogany at top; but both taken together do not exceed 1¼ inch in thickneſs.

The nature of the moveable ſtands, whereof there were at laſt no fewer than ſeventeen provided, will be comprehended from the repreſentations of them towards the right-hand in tab. XVIII. and XIX. Their general conſtruction, in what regards the part of them which is fixed, differs not from that of the others, excepting that they were of different heights, from two feet to about two feet eight inches, ſo as better to ſuit the irregularities of the ground where it might be neceſſary to place them. In the middle of each of theſe, an hexagonal wooden pipe deſcends, from the top to within two or three inches of the bottom, where it is joined by a brace reaching from each leg. This pipe receives the common cheeſe-preſs wooden ſcrew (having three ſides ſcrewed and three plane), to the top of which the ſquare table is attached. It is embraced by the circular nut, or winch with four handles, whereby the table is elevated or depreſſed at pleaſure; and being brought to its proper height, is there made perfectly faſt by means of the flat-headed iron ſcrew, which paſſing through one of the legs, preſſes an iron plate, fixed in the inſide of the pipe, againſt one of the plane ſides of the ſcrew.

In deſcribing the deal rods, there has already been occaſion to make mention of the vertical and horizontal clamps, whereby the croſs-feet are faſtened to the table on the top of the ſtand. The nature of theſe tables will be beſt underſtood by conſulting the two plans of them towards the right-hand in tab. XVIII.; whereof one repreſents the two grooves fitted for the alternate reception

of the horizontal clamp, according to the ſide on which the rod lies that is to be moved on into coincidence; and the other ſhews it actually in its place, with the clamp itſelf detached in elevation along-ſide of it. Thus from the plan it may be perceived, that the firſt, or adjuſted rod, lies towards the farther ſide of the table, and is there ſecured by the vertical clamp. The ſecond, or moveable rod, lies on the hither ſide, and therefore the horizontal clamp is placed in the farther groove, where it is firmly pinched by the nut underneath. The rod has been brought to coincidence by working with the two milled-headed ſcrews againſt the oppoſite ſides of the croſs-foot. This apparatus, although perfectly good in theory, was found to be much too confined in its nature to anſwer well in practice, requiring the ſtands to be placed with a degree of preciſion, which could not be effected in the field without great loſs of time; and this was the real cauſe, as will be ſeen hereafter, that the meaſurement by coincidences with the deal rods was given up, and that by contacts adhered to.

Towards the left-hand of tab. XVIII. the plan of one of the ſquare tables is repreſented with the ends of the ſecond and third rods upon it in contact. In this operation it will be perceived, that only one croſs-foot of each rod could now reſt on and be clamped to the ſtand, the tables having been inadvertently cut too ſmall to admit of both; and although this has the appearance of imperfection, yet no inconveniency whatever was found to reſult from it in practice, experience having ſhewn, that the clamping of either end ſufficed to keep the rod ſteady. Along-ſide of the table, the vertical clamp, being that now ſolely made uſe of, is likewiſe repreſented in elevation.

On the face or exterior ſide of each leg of all the ſtands, fixed as well as moveable, a plate of braſs is ſcrewed near the bottom, with two holes in each, over a groove purpoſely made in the wood underneath. By means of theſe plates, parallelopid leaden weights, about fourteen pounds each, having braſs pins with heads ſuited to enter the holes, and fall down in the grooves, into a narrow-pointed part of them, are readily ſlipped on or off each leg. Thus every ſtand, excluſive of its own weight, which is about thirty-one pounds, being loaded with forty-two pounds of lead, is thereby rendered perfectly firm and ſteady.

A number of wedges were alſo prepared, and always ready to be placed under the legs; by means of which, and a ſpirit level laid on the table, its plane is brought to the proper poſition.

Notwithſtanding all theſe precautions, it having been found, in the meaſurement with the deal rods, that time was loſt in levelling the ſtands, particularly in ſituations where the ſurface happened to be more than uſually uneven, or where it was of a looſe or ſpungy nature; therefore Mr. Smeaton adviſed (and no man's advice is more deſerving of attention), that deal platforms, ſtanding on pickets driven into the ground, and properly levelled, ſhould be uſed to receive the legs of the ſtands. Accordingly, for the operation with the glaſs rods (table XIX.) twenty ſuch triangular platforms made of inch deal, whoſe ſides were each three feet two inches in length, and void in the middle, were provided; as alſo a number of beech-pickets, about an inch and a half ſquare, and of different lengths, from ſeven to twelve or fourteen inches. Three of theſe pickets, ſhort or long as the ſituation required, being driven into the ground, till their heads (by the carpenter's

level) were brought to the proper height, the platform was laid upon them; and on that the ſtand itſelf being placed, its poſition was ultimately corrected by the ſpirit level laid on the top of the table. Each of the beech pickets had a hole bored through its top, fit to receive a piece of ſtrong tent-line, by which, and the help of one of the camp mallets, the pickets were eaſily pulled up again, when the platform was to be removed to a new ſituation.

Boning Teleſcope and Rods. Tab. XVIII.

In order to trace the line of 200 yards or 600 feet through the air, from one fixed ſtand to the other, it was uſual, in the firſt place, to ſtretch a cord extremely tight along the ground, and to divide the ſpace into rod lengths, by ſmall wooden pins placed cloſe by the cord, which remained there, and accordingly marked, very nearly, the points over which the centers of the intermediate ſtands were to come. A piece of wood, about fourteen inches in length, and one and a half in breadth, painted white, with a narrow black line along the middle of it, being prepared for the purpoſe, was laid on the ſurface of the farther ſtand. The boning teleſcope, fourteen inches long and one and a half in diameter, with a ſmall magnifying power, and moveable object-glaſs, ſo as to fit it for very ſhort diſtances, was then laid on the ſurface of the neareſt ſtand; which, by means of wedges placed under the legs, had that ſide towards the farther ſtand ſo elevated or depreſſed, as to bring the croſs wires to coincide with the black line on the painted board. Twenty-four boning rods had been originally provided; but it rarely happened, that more than eight or ten

of

of that number were uſed in any one ſtation. They are of clean deal, upwards of five feet in length, one inch ſquare, and pointed with plate iron at the bottom, ſo as to be eaſily fixed into the ground. Each rod carries a croſs vane, ſix or ſeven inches in length, and three-quarters of an inch in breadth. This croſs vane, being moved upwards or downwards along the rod, till its upper ſurface coincided with the croſs wires of the teleſcope and black line on the painted board, its under ſurface then marked the height to which the ſurface of the ſtand was to be brought at that particular place. In this manner, a certain number of points, in the line paſſing through the air from one fixed ſtand to the other, being accurately obtained, it was very eaſy, at all the intermediate places, by the application of the eye alone to the ſurface of any one ſtand or rod, to bring the ſurfaces of the other ſtands near it into the ſame plane.

Cup and Tripod for preſerving the point upon the ground, where the meaſurement was diſcontinued at night, and reſumed next morning. Tab. XVIII.

It has been already mentioned, and, in giving the account of the rough meaſurement with the chain, there will be farther occaſion to remark, that the baſe was divided into hypothenuſes of 200 yards or 600 feet each, where ſquare pickets were driven into the ground, and regularly numbered, ſo as to be eaſily referred to on any occaſion. In the meaſurement with the rods, it was cuſtomary to finiſh the day's work at or near one of theſe ſtations. When the rods of twenty feet were uſed, the termination of a rod was, of courſe, always found

found to be within a few inches of the picket correſponding with the hypothenuſe, as determined by the chain. But with the rods of twenty feet three inches, the day's work was always ended with a fractional rod, by ſuſpending a plummet from ſome convenient part of the ſtem, marked for the purpoſe, and which conſequently became the point of commencement next morning.

The braſs cup, made uſe of on theſe occaſions, is of the figure of an inverted truncated cone, whoſe mean diameter is four inches, and its depth about five, with a very ſmall inclination in the ſides. It was placed in a hole dug for it in the earth, immediately under the point of ſuſpenſion of the plummet, ſerving only to hold the water in which it vibrated.

The nature of the tripod will be beſt conceived from the plan and elevation of it in tab. XVIII. It conſiſts of two ſtrong pieces of beech wood, mortoiſed into each other, ſo as to reſemble a half croſs, or the letter T inverted, having three ſtrong iron prongs, about twelve inches in length, which paſs through the ends of the wood, and are faſtened to it by ſquare nuts at top. On the ſurface of the tripod lies a ſimilar half croſs of mahogany, moveable by means of grooves in the direction of the longeſt ſide, and fixable by its proper ſcrews, when brought to the deſired poſition. This mahogany half-croſs carries on its ſurface a braſs ruler, moveable at right-angles to the former direction, fixable alſo by means of its own ſcrews, and on whoſe end is cut a very fine interſection. Thus any day's operation having been finiſhed, the tripod was placed near the cup, with it longeſt ſide parallel to the line of meaſurement, and its prongs driven into the ground, ſo as to be rendered perfectly immoveable without great violence. The plummet being then ſuſpended by a fine gilt wire, at any part

of

of the ſtem of the deal rods indifferently, but always at the fixed * or hindermoſt end of the glaſs rods, the braſs ruler was advanced ſo near as almoſt to touch the wire, and there made faſt. This being done, the mahogany half-croſs was laſtly moved backwards or forwards, in the direction of the line of meaſurement, until the interſection, as ſeen by a perſon lying down on the ground for the purpoſe, accurately coincided with the gilt wire, where it was likewiſe faſtened by its proper ſcrews. A tent was then pitched very near the apparatus, for the ſoldiers who furniſhed the centinel for its ſecurity, till the meaſurement was reſumed; and particularly to guard it from being diſturbed by cattle during the night.

Wheels for terminating, in a permanent manner, the extremities of the Baſe. Tab. XVIII.

Before any accurate meaſurement could ultimately be made of the baſe by means of rods, in order that we might with certainty refer to the ſame point, on any occaſion that might ariſe of correction or repetition of the work, it had all along been foreſeen, that it would be abſolutely neceſſary to ſink deep into the ground wooden pipes, or ſuch like things, at the extremities of the baſe, which could not be removed, or even diſturbed, by idle or ignorant people, without very conſiderable labour. Mr. MYLNE, F.R.S. was accordingly requeſted to

* That this might be conveniently done, a moveable ſtand was placed, under the glaſs rod, about four feet from the fixed end, and its table elevated till, by bearing againſt the lower part of the caſe, it received its weight. This permitted the ſtand under the fixed end to be lowered and removed, to make room for the apparatus.

order

order two ſuch pipes to be provided, about ſix feet in length each, and one foot in diameter, with a bore of four inches in the uppermoſt end, for the depth of two feet, and croſs-arms near the lowermoſt end, in the ſtile of the common warping poſts. As an improvement on this idea, Mr. MYLNE very judiciouſly propoſed that, inſtead of the croſs-arms, the lower ends of the pipes ſhould paſs through the nave of an old coach-wheel, and then be ſecured by a bolt underneath. This alteration was approved of; and the machines, thus executed, were ſent ſoon after by water to Hampton.

The plan and ſection of one of theſe wheels, with the diſhed ſide downwards, are repreſented towards the left-hand in tab. XVIII. where it will be perceived, that by means of four knee-pieces, made of crooked oak, the pipe is firmly bolted to the wheel, and thereby kept at right-angles to its plane. The top of the pipe is alſo ſecured exteriorly by an iron hoop, and has a caſt-iron box driven into it, whoſe inner diameter is four inches, anſwering to that of the bore. Four oak piles for each wheel were prepared to be driven into the bottoms of the pits dug for their reception, which were ſix feet in diameter, and the ſame in depth. The ſoil near Hampton Poor-houſe being of a looſe ſandy nature, there the piles were eaſily driven into the bottom, until their tops were on the ſame level. The flat of the fellies of the wheel being then laid on the piles, the earth was filled in and well rammed around the pipe, quite up to the ſurface, with which its mouth is even. But the ſoil at King's Arbour, being a hard-bound gravel, the piles could not be driven into the bottom of that pit; wherefore, the flat of the wheel reſts there on the gravel only.

The braſs cup, formerly deſcribed, was from the firſt intended to be placed in the pipes, for which purpoſe it has two

lids; one a ſemi-circle, with the central point marked by a line cut on its diameter, brought into the direction of the baſe; with which line the gilt wire, ſuſpended at the extremity of the firſt rod, was made to coincide on the commencement of the meaſurement. The other lid has a very ſmall hole made in its center, through which the plummet wire is to paſs, when ſuſpended from the center of the inſtrument, hereafter to be made uſe of for the determination of the angles at the baſe, or in any other ſtation whatever, where it may be neceſſary to bring it very accurately over a point on the ſurface of the ground underneath.

Rough meaſurement of the Baſe with the Chain, and determination of the relative heights of the Stations by means of the Teleſcopic Spirit Level. Tab. XVI. and XVII.

Having in the preceding deſcription of the various inſtruments, originally provided for the meaſurement of the baſe, fully explained their conſtructions, uſes, and modes of application; and having thereby anticipated, in a great degree, what muſt otherwiſe have been ſaid to make them underſtood in any account, blended with that of the execution; little more now remains to be given than the journal of our proceedings from day to day, and the ultimate reſult of the operation.

After a very tedious delay, Mr. Ramsden having at laſt produced his hundred-feet chain, with the portable tranſit inſtrument; and having lent us an excellent teleſcopic ſpirit level, for determining the relative heights; two ſections of the baſe being likewiſe cleared by the ſoldiers, and ſome progreſs made in

the third, we found ourſelves, on the 16th of June, in readineſs to begin the rough meaſurement.

Lieut. Colonel CALDERWOOD, of his Majeſty's Horſe-Guards, F.R.S. had, from the beginning, been ſo good as to promiſe his aſſiſtance in the operation. Lieut. Colonel PRINGLE too, of the Corps of Engineers, obligingly became a volunteer on the occaſion; as did alſo Mr. LLOYD, F.R.S. a few days afterwards; while Enſign REYNOLDS, of the 34th regiment, who had for ſome time paſt been employed in ſurveying the environs of the Heath, continued that work with ſuch ſpare hands as could be afforded him for that purpoſe; and it is to the plan (tab. XVI.) done by that officer, that it will be neceſſary to refer in any thing regarding locality, in what has hitherto been ſaid, as well as in the ſubſequent relation.

The lower end of the baſe had for ſome time paſt been diſtinguiſhed by a St. George's flag fixed to the top of a fir ſpar, thirty-five feet in height; and one of the ſignal bell-tents ſtill remained at the ſtation near the ſummer-houſe. A rope of 200 yards being made very faſt by a ſtrong iron picket, driven into the ground at the bottom of the flag-ſtaff, the other end was carried on along the baſe, and placed at the bottom of a camp-colour, in a line with the bell-tent. The rope being wound around a ſtrong iron reel, prepared for the purpoſe, was thereby ſtretched extremely tight, a perſon occaſionally lifting it up in the middle, or at other places, and letting it drop again, ſo as to bring the whole into the ſame ſtraight line. Five perſons were neceſſary for the proper management of the chain; two at each end for its adjuſtment there, and one towards the middle, to lay it cloſe to the rope, or to bear it up in any particular place, where the circumſtances of the ground rendered ſuch precautions uſeful. The zero or rear end of the chain

being ſtrained back ſo as to coincide with the point of commencement, a ſteel arrow was placed as erect as poſſible in the ſemi-circular cavity of the braſs handle at the other end. The chain being then drawn on, till the cavity in the rear handle could be applied to the firſt arrow, a ſecond was then placed in that of the front handle, and ſo on until ſix chain lengths were thus meaſured off; which terminating the firſt hypothenuſe, a beech picket, ſomething more than an inch ſquare, and about ſeven in length, with N° 1. cut upon it, was driven into the ground, till its head was nearly level with the ſurface. It is however to be remarked, that the ſixth arrow of each hypothenuſe was conſtantly left in the ground till the firſt of the ſucceeding one was placed, to avoid the error that would have otherwiſe ariſen in applying the rear end of the chain to the picket inſtead of the arrow.

In this manner we proceeded on the 16th of June, and in the ſpace of about three hours and a half, completed the firſt meaſurement of the ſouth-eaſt ſection of the baſe, comprehending the thirteen hypothenuſes between the flag-ſtaff and ſtation near Hanworth Summer-houſe, the diſtance being 78 chains or 7800 feet, making 2600 yards; and the mean temperature of the air being 63°.

On the ſubſequent day this ſection was re-meaſured with equal care, when the total extent fell ſhort of the thirteenth picket only five inches. And here it is to be obſerved, that a conſiderable part of this difference probably aroſe from the ſtretching of the chain acroſs Wolſey River, at the ſame time that the irregularities of the ground are greater in this than in either of the other two ſections. The mean heat of this day was 65°.

The

The operation with the chain was ſuſpended during the 18th and 19th of June, thoſe days having been employed in ſettling certain matters with Mr. RAMSDEN relative to the deal rods, as well as to give time for the making of a holdfaſt for the rear end of the chain, invented by Lieut. Colonel PRINGLE. This machine, whereof the plan at large is repreſented by dotted lines at the handle of the chain, as it is in ſmall by the two elevations adjoining in tab. XVII. conſiſts of a ſemi-circular iron plate, from the bottom of which projects two double and one ſingle prong. In the middle, between the two double prongs, a ſemi-circular cavity is formed, fitted to receive the ſteel arrow on one ſide, while that in the braſs receives it on the other. In a ſocket in the middle, a ſtrong wooden handle, reſembling that of a ſpade, is placed. Thus the rear handle of the chain being applied to the arrow, the holdfaſt embraces with its double prongs the ſtraight part of the braſs, and in that poſition, being forced into the ground by the action of a man at the handle, the rear end of the chain is thereby kept ſo firm as to be immoveable by the efforts of the two men at the other end, in ſtretching it to its true poſition, for the front arrow.

On Monday, the 21ſt of June, the operations were reſumed, by meaſuring twice with the chain (forwards and again backwards) the thirteen hypothenuſes comprehended in the ſecond ſection of the baſe, between Hanworth Summer-houſe and the north-weſt bank of the great road (an old Roman way) leading from Staines to London. This being the ſmootheſt part of the Heath, and the holdfaſt being now applied, the two meaſurements differed only one inch and a half in the diſtance of 7800 feet. This inſtance of accuracy is alone ſufficient

ficient to prove the great excellence of the chain, although another will be given hereafter ſtill more ſurpriſing.

On the ſame day that the ſecond ſection of the baſe was meaſured, the levels of that and the firſt were taken. The operation of levelling is ſo univerſally known, as to render any detail of it unneceſſary. It will be ſufficient to ſay, that the ſpirit level made uſe of on this occaſion was a very good one, about eighteen inches in length, and could at all times be very readily and accurately adjuſted by inverſion in its Y's. The tops of the pickets, marking the hypothenuſal diſtances, were the points on which the levelling rods were placed on each ſide of the level; which being inverted at the intermediate picket, points equi-diſtant from the center of the earth were thereby obtained, at the croſs vanes of the levelling rods, and no correction for curvature or refraction neceſſary. It will be readily underſtood, that the relative heights of the pickets were found by meaſuring their diſtances from the centers of the croſs vanes and axis of the teleſcope reſpectively.

The ſix firſt columns towards the left-hand of the firſt or general table ſubjoined to this Paper, ſhew diſtinctly every thing relating to the levels of the whole baſe, thoſe of the third ſection having been determined on the 22d of June. By examining the table it will be ſeen, that the aſcent on the firſt ſection is 10.555 feet,

on the ſecond	.	8.580
and on the third	.	12.130
Total	. .	31.265 feet, be-

tween the lower extremity at Hampton Poor-houſe, and the higher near King's-Arbour.

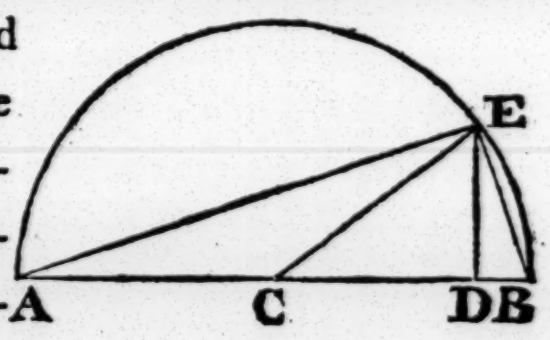
The

The computed numbers in the ſeventh column are the reductions * depending on the aforeſaid heights, or the differences between the hypothenuſal diſtances of 600 feet each and the reduced baſe diſtances. With regard to the remaining columns of the table, or thoſe towards the right-hand, they will be ſeverally ſpoken to hereafter, in taking into conſideration the expanſion of metals, as determined with great accuracy by the experiments with the pyrometer.

Hitherto no uſe had been made of the tranſit inſtrument: for, in order that it might be applied to advantage, there was a neceſſity for laying the wheel into the ground at the lower end of the baſe, and ſo to modify the St. George's flag-ſtaff that, being placed in the pipe, it might be ſteadily ſupported by braces in a true vertical poſition; which we found, from experience, could not be effected by ropes only.

The wheel being accordingly laid in its place, and the other precautions taken for ſecuring the flag-ſtaff, which was likewiſe painted white, that it might be more diſtinctly ſeen from

* The reduction in the ſeventh column, I have computed by the difference between the ſquare of the hypothenuſe, actually meaſured, and the ſquare of the height found by the level; and Lieut. Colonel CALDERWOOD has done the ſame thing by a much ſhorter method. Thus, in the annexed figure, CE being the hypothenuſe of 600 feet, DE the perpendicular height obtained by levelling, DB the reduction required, or the difference between the hypothenuſe and true baſe; then, ſubſtituting the chord BE inſtead of DE, the following analogy is obtained; AB : BE ::



BE : DB; conſequently, $\frac{BE^2}{AB} = DB$: that is, the ſquare of the perpendicular height being divided by double the diſtance, or 1200 feet, the quotient is equal to DB the reduction, without ſenſible error. For if DE were four feet, the greateſt perpendicular height in the baſe, BE the chord would only exceed it $\frac{1}{200000}$, which would not be more than $\frac{1}{40000}$ part of an inch. The difference between the reſults, by the two modes of computation, is ſo trifling as not to deſerve notice.

the

the farther extremity; on the 22d of June, the tranſit inſtrument was adjuſted over the thirteenth picket at Hanworth Summer-houſe, while directed upon the flag-ſtaff. But it being now found, that the vertical plane paſſing through the flag-ſtaff fell to the eaſtward of the center of Banſted Spire, therefore the tranſit was gradually moved to the eaſtward, until by repeated trials the three points were perceived to be in the ſame vertical plane, when the picket was moved, and re-placed exactly under the axis of the teleſcope, a few inches from its firſt poſition. The ſame operation was repeated at the twenty-ſixth ſtation, on the farther bank of the Staines Road; and, laſtly, at the forty-ſixth, forming the north-weſt extremity of the baſe; where a pit was immediately dug for the wheel, which was placed therein, without however filling in the earth for the preſent, that being deferred till near the completion of the meaſurement with the deal rods. Thus the two extremities, and two intermediate points of the baſe, being accurately placed, by the help of the tranſit inſtrument, in the ſame vertical plane with Banſted Spire, it was eaſily ſeen, that by arranging camp colours in the intervals at any time, all the other points might be brought ſo nearly to coincide with theſe firſt, as not to occaſion, by deviation, any ſenſible error in the meaſurement afterwards to be made. This application of the tranſit ſhewed us, however, that ſome labour had been loſt by not uſing it ſooner: for at the Staines Road, the tract cleared by the ſoldiers deviated about two feet and a half too much to the weſtward for the true line; and at King's Arbour it was twice as much; ſo that we were now obliged to widen the cleared tract, by adding to the eaſtern ſide of it.

On the ſame day that the chief points in the baſe were fixed by means of the tranſit, and the levels of the third ſection

taken

taken as before-mentioned, the rough meaſurement of that ſection with the chain was completed, and found to contain nineteen hypothenuſal diſtances of 600 feet each, and one of 404.55, making in the whole 11804.55 feet, between the twenty-ſixth ſtation at the Staines Road and the center of the pipe near King's Arbour, the mean temperature being 62°½. Here it is to be obſerved, that this laſt ſection was only meaſured once with the chain, the tract not being yet ſufficiently cleared to admit of its being done to the beſt advantage; and, when completed, it was judged to be better to proceed directly in the operation with the rods, than to loſe time in the uſual repetition, ſince the merits of the chain, in this way of applying it, were already ſufficiently well eſtabliſhed; and any future teſts to which it was to be put were propoſed to be of a more rigid nature.

When the length of the chain, in its original ſtate, was aſcertained by the dots on the braſs pins in the New-England plank, it was found, in the then temperature of 74°, to exceed the 100 feet by near one quarter of an inch, or 0.245 inch. Therefore, in the temperature of 63°, being that in which the lengths of the deal rods were laid off, and differing very little from what was likewiſe the mean heat of the air, when applied upon the Heath, the chain, according to the experiments on the expanſion of the very ſame ſteel, would exceed the 100 feet by 0.161 inch, or 0.0134 foot. Hence the ſum of the three ſections of the baſe, 274 chains, being multiplied by 0.0134 foot, we ſhall have 3.67 feet for the equation of the chain +4.55 feet, to be added to its length, which will then become 27408.22 feet from the center of one pipe to the center of the other: and this would have been the true length of the baſe, as given by the rough meaſurement with the

chain, if the ſurface had been one uniform inclined plane throughout its whole extent. But, although the aſcent of Hounſlow-Heath is ſo ſmall, and ſo gradual, as to occaſion little more than half an inch of reduction, from the 46 hypothenuſal to the 46 baſe diſtances, into which it is divided, as may be ſeen by referring to the table; yet each of theſe hypothenuſes containing again many other ſmall irregularities, all of which affect the meaſurement by the chain, in proportion to their number and height, in every ſpace of 600 feet, their united effects, including the lateral deviations from the true line in meaſuring, do ſomewhat more than compenſate for the extra-length of the chain, as will be ſeen hereafter in comparing the length of the baſe juſt now obtained with that given by the rods.

The weather, which during the greater part of June had been wet, became ſtill worſe towards the end of the month and firſt week of July; ſo much ſo, that even if the deal rods had been ready they could not have been uſed with advantage. The ſoldiers, nevertheleſs, were not idle, being, when the weather would permit, partly employed in clearing the Heath, and partly in aſſiſting Mr. REYNOLDS in the ſurvey, towards the perfecting of which many chief points were fixed by means of my aſtronomical quadrant, placed for that purpoſe at ſeveral different ſtations of the baſe. At this time too (July 8th) I levelled from the lower end of the baſe to the ſurface of the Thames at Hampton, and found the deſcent to be 36.1 feet.

Measurement of the Base with the Deal Rods. Tab. XVI. and XVIII.

Such extraordinary care and pains had been bestowed in the construction of the deal rods, in order to render them the best which had ever been made, that, although begun early in June, they were not completely finished before the 15th of July. They were brought that afternoon by Mr. RAMSDEN, together with the various parts of the apparatus necessary for their application in the field, to the camp now moved from Hanworth Summer-house to the intersection of the base with Wolsey River; whence they were transported, early next morning, to the pipe near Hampton Poor-house, where we were met by Sir JOSEPH BANKS, accompanied by Messf. BLAGDEN, CAVENDISH, LLOYD, and SMEATON, all ready to lend their assistance in the subsequent mensuration.

Before I proceed farther, I think it here incumbent upon me very gratefully to remark, that the respectable and very worthy President of the Royal Society, ever zealous in the cause of science, and who had repeatedly visited the heath, to offer aid, if such had been necessary, while the first and rougher part of the operations were going on; now, that others of a more delicate nature were to commence, and where it was of importance, that those entrusted with the execution should meet with as few, and as short, interruptions as possible, not only gave his attendance from morning to night in the field, during the whole progress of the work; but also, with that liberality of mind which distinguishes all his actions, ordered his tents to be continually pitched near at hand, where his immediate

gueſts, and the numerous viſitors whom curioſity drew to the ſpot, met with the moſt hoſpitable ſupply of every neceſſary and even elegant refreſhment. It will eaſily be imagined, how greatly this tended to expedite the work, and how much more comfortable and pleaſant it rendered the labour to all who obligingly took part in it; but more eſpecially to him, who, being a volunteer in it at firſt, conſidered himſelf as bound to perſevere in his beſt endeavours to bring it to a ſucceſsful concluſion.

From the deſcription that has been given of the deal rods, it will be remembered, that they are fitted to be applied in meaſuring, either by the coincidences of lines, inlaid one inch and a half from each extremity, or by the contacts of the ſpherical lips of the bell-metal with which they are tipped. The firſt, ſeeming to be the moſt accurate, although the moſt tedious method, was that by which we propoſed to ſet out.

The flag-ſtaff having been previouſly removed from the pipe, and the braſs cup filled with water put in its ſtead, all the neceſſary precautions being likewiſe taken for preſerving the line of direction, horizontally, by the rope ſtretched along the firſt hypothenuſe, and vertically, by means of the boning rods; the firſt ivory line on the firſt rod was brought by the plummet to coincide with the center of the cup, in which poſition, being clamped, it accurately marked the commencement of the baſe. The ſecond rod being now applied to the firſt, and moved up by the apparatus formerly deſcribed (tab. XVIII.) till its line coincided with that on the firſt; and, in like manner, the third rod being applied on the alternate ſide of the ſecond, moved up and clamped as the reſt; thus the exact diſtance of ſixty feet was aſcertained, care being always taken, that the firſt adjuſtments were not diſturbed, while the

ſubſequent

ſubſequent ones were forming. The clamps faſtening the firſt rod to its ſtands being then detached, it was carried by two men and laid on the alternate ſide of the third; and ſo on in ſucceſſion, until fifteen rod lengths were meaſured off, being the half of the firſt hypothenuſe.

The time conſumed in meaſuring this ſhort diſtance of 300 feet was not leſs than five hours; owing, as has been formerly mentioned, to the confined nature of the apparatus for moving the rods on into coincidence, which required ſuch nicety in placing the ſtands, as could not be effected until after ſeveral repeated unſucceſsful trials. All the executive people were therefore of opinion, that it would be proper to diſcontinue this mode of meaſurement, at leaſt until a more convenient apparatus could be thought of for the purpoſe; and that, in the mean time, we ſhould proceed by the method of contacts, as the only alternative we could for the preſent adopt *.

The rods being accordingly placed in contact with each other, we ſoon made greater progreſs, finiſhing the operations of the day at the middle of the fourth hypothenuſe, where the tripod, with its guard, was placed, to preſerve the point of commencement for the enſuing morning.

* Although I acquieſced in the change thus become neceſſary, yet it was with much reluctance, becauſe it left undecided the conteſted point, with regard to coincidences and contacts. If we could have proceeded with the coincident rods till eighty-one lengths were meaſured off, and then meaſured back the ſame ſpace by placing eighty rods in contact, the point would have been clearly ſettled. For if the termination of the eightieth rod agreed exactly with the point of departure, contacts being the moſt expeditious would have been judged the beſt method. On the contrary, if the eightieth rod fell ſhort of reaching the point of departure, there could have been no doubt, that the difference muſt have ariſen from butting one rod againſt the other, whereby a certain ſmall proportion of each rod came to be loſt in the account, by being meaſured twice over.

The

The meaſuring rods, when put into the cheſt in London, had been compared and found to agree with the ſtandard. The compariſon was not repeated on the 16th; but this being done on the 17th, at 7 h. A.M. under the oil-cloth canopy at the camp, they were found at a medium to exceed the ſtandard by one-fiftieth of an inch, the temperature then being 62°. After the compariſon they were carried to the place of the tripod, when the operation was reſumed by bringing, with the help of the plummet, the ſame point of the rod with which we had left off work, to coincide with the interſection on the braſs ruler. The meaſurement of this day was cloſed at the end of the tenth hypothenuſe, when the rods being carried back to camp, were compared, and found accurately to agree with the ſtandard.

A conſiderable fall in the barometer, between the evening of the 17th and the morning of the 19th, portended rain. Nevertheleſs, all parties repaired to the place of rendezvous, which was appointed at the lower end of the baſe, in order to re-meaſure the two firſt hypothenuſes, by placing all the rods in contact, which on the 16th had been done partly one way and partly the other. The operation being according repeated with great care, the point of the ſixtieth rod, which formerly correſponded to the center of the ſecond picket, was now found to be puſhed forward exactly forty-five inches, anſwerable to the deficiency on the fifteen coincident rods, with which the menſuration was begun. It now began to rain, therefore the rods were carried back to camp, and being ſeverally compared, they were found to exceed the ſtandard each by one-thirtieth of an inch, occaſioned by the extraordinary humidity of the air. A heavy rain enſued; and what made this much more regretted by all was, that in the forenoon their Majeſties graciouſly condeſcended to honour the camp with their preſence, and continued

tinued there ſome time; but the weather becoming rather worſe, it was utterly impoſſible to ſhew their Majeſties the nature of the operation, by any progreſs that could at that time be made in the work.

After a continuance of unfavourable weather for ſeveral days, the operations were reſumed at 9 h. A.M. of the 23d, when the rods being compared were found ſtill to exceed the ſtandard by one-thirtieth of an inch, and the temperature now was 61°. Here it is to be obſerved, that in our progreſs forward, an accurate regiſter had been all along kept of that point of each rod correſponding to the center of the hypothenuſal pickets, by noting its diſtance from either end, whereby the error of the chain at each ſtation was readily diſcovered, at the ſame time that the revolutions of the three rods ſerved to keep the account of the total meaſurement. In order, therefore, that this method might be diſtinctly adhered to, it was judged proper to puſh on the rod that lay over the tripod at N° 10. exactly forty-five inches, to make good the deficiency of the firſt fifteen coincident rods, and that the account might be kept from the lower end of the baſe in entire rods of 243, and complete revolutions of 729 inches each. This being done, the reſt were placed in the ordinary ſucceſſion; and we finiſhed the buſineſs of the day at the eighteenth ſtation, where the rods being compared at 6 h. P.M. their mean length was found to exceed that of the ſtandard $\frac{1}{67}$th part of an inch, the temperature then being 54°.

On Saturday the 24th of July, the rods were three times compared; at 7 h. 30′ A.M., 11 h. 15′ A.M., and 5 h. 45′ P.M. Their mean exceſs above the ſtandard was found to be one-thirtieth of an inch, and the mean heat 64°. In the courſe of the day, the meaſurement was continued from the eighteenth to the

the twenty-ſeventh ſtation, or firſt of the third ſection of the baſe, where the tripod was placed as uſual; and there it remained untouched, on account of bad weather, till Monday the 2d of Auguſt.

Conſidering how much time and labour had been beſtowed in obtaining what we certainly had every reaſon to conclude were the beſt deal rods that ever were made, it was no ſmall diſappointment now to find, that they were ſo liable to lengthen and ſhorten by the humid and dry ſtates of the atmoſphere, as to leave us no hopes of being able, by their means, to determine the length of the baſe to that degree of preciſion we had all along aimed at. But ſince more than one-half of it was already meaſured, it was judged proper to proceed with them in their preſent ſtate, and then to have them carefully painted or varniſhed, before they ſhould be farther uſed.

The unfavourableneſs of the ſeaſon, and delays in obtaining the inſtruments, had already been the cauſes of protracting the operations on Hounſlow-Heath greatly beyond what was at firſt expected; and the failure of the deal rods gave no immediate proſpect of their being ſpeedily brought to a concluſion. On revolving in my own mind the different alternatives we might ultimately be obliged to have recourſe to, metal rods of ſome kind or other, whoſe expanſion could always be determined by experiment, ſeemed to promiſe a reſult that might be ſafely relied on. Caſt iron was what I had thoughts of propoſing, knowing from an experiment which I had made myſelf, that it expanded leſs than ſteel. The cumberſomeneſs of its weight appeared indeed objectionable; but that inconvenience was either to be ſubmitted to, or one of another kind, namely, the reduction of the length, which was always, if poſſible, to be avoided.

At this time Lieut. Colonel CALDERWOOD could not conveniently lend us his assistance in the field; but he visited us occasionally, and on one of these visits proposed to me, that glass rods should be made use of instead of deal; putting me in mind of another experiment * that I had made, which seemed to shew that solid glass rods expanded less than tubes. This proposition the Lieutenant Colonel, before he came to the heath, had made to Mr. RAMSDEN, who appeared averse from making the trial, because of the great length of the rods, and the brittleness of the material. Nevertheless, it being sufficiently obvious, that glass rods or tubes of the full length, or something approaching towards it, would be much sooner provided than any metal rods whatever, and the saving of time being a point of consequence; Lieut. Colonel CALDERWOOD was accordingly requested to make the trial at the glass-house, as soon as possible after his return to town. Next day he succeeded in getting a fine tube drawn, eighteen feet long, and about one inch in diameter; and there seemed to be no longer any doubt, that those of the proper length might be obtained. It was found, that solid glass rods of such extraordinary dimensions could not be had, it being impossible to take at once a sufficient

* The experiment here alluded to was made with Mr. CUMMING's pyrometer, which from its construction did not admit of a very accurate estimation of the heat communicated to the standard bar, the rod, and tube respectively. Either, therefore, the natures of the glass rod and tube, made use of at that time, must have been very different, to cause the difference of expansion; or some circumstance in the instrument unattended to had occasioned the fallacious appearance: for it will be found, from the experiments hereafter to be given in detail, that a solid glass pendulum rod expands fully as much as, nay in this particular instance even more than a tube; but different glass, having different specific gravities, will no doubt be susceptible of different degrees of expansibility.

quantity of the melted metal on the irons, made uſe of for drawing them at the glaſs-houſe.

The week of rainy weather, which ended the month of July, occaſioning, as has been ſaid, a total ſuſpenſion of the operations on the heath, was employed in procuring a ſufficient number of glaſs tubes (one whereof was not leſs than twenty-ſix feet long) and regulating with Mr. RAMSDEN every thing concerning their conſtruction into meaſuring rods. The deſcription of them we ſhall however defer until the time of their application in the field, after having finiſhed the operation with thoſe of deal.

On Monday the 2d of Auguſt, the operations on the heath were reſumed at 8 h. 30′ A.M. by comparing the rods with the ſtandard, which they were found to exceed by one-fortieth of an inch, the temperature then being 66°. The forward end of the rod now placed over the tripod at N° 27, completing the 800th length, reckoned from the lower end of the baſe by rods of 243 inches each; and theſe being equal to 810 rods of 240 inches; it was judged proper to mark a point upon the ground correſponding to this forward end, that it might be referred to in returning back with the meaſurement by the glaſs rods. This was done by ſinking two ſmall pickets into the ground, about a foot aſunder, one on each ſide of the baſe, and at right angles to it. A ſilk thread being then ſtretched over the tops of the pickets, and gently moved on till it touched the ſilver wire ſuſpended from the end of the rod, fine notches were then made with a pen-knife in the tops of the pickets, whereby the thread could be replaced in the ſame ſituation; which being done, the pickets were covered over with earth. In the courſe of this day nine hypothenuſes were meaſured; and at 7 h. P.M. the tripod was placed at the thirty-

ſixth

ſixth ſtation. The rods, being now compared, were found to agree with the ſtandard; and the temperature was $67°\frac{1}{2}$.

On Tueſday the 3d of Auguſt, the rods were compared at 7 h. A.M. and found only to exceed the ſtandard by one-ſixtieth of an inch. Being arrived at the middle of the forty-firſt hypothenuſe, a point correſponding to the forward end of the 1215th rod was transferred to the ground by the double pickets and ſilk-thread, as had been done at the twenty-ſeventh ſtation. The meaſurement was then continued to the north-weſt extremity of the baſe, which was found in the whole to contain 1353 complete long rods of 243 inches each +21 inches, where the tripod was placed, in the point which of courſe correſponded to the 1370th ſhort rod of 240 inches each, equal to 328800 inches, or 27400 feet. To which diſtance we have yet to add 4.31 feet, being the ſpace intercepted between the interſection on the tripod and the center of the pipe marking the north-weſt extremity of the baſe; whoſe total length, as given by the deal rods, without regard to expanſion, or reduction of the hypothenuſal line, becomes 27404.31 feet. And here it is to be obſerved, that the interſection on the tripod terminating the 27400 feet only over-ſhot the picket anſwering to the 274th chain by two inches and nine-tenths. But this nice agreement between the reſult by the deal rods, and that furniſhed by the rough meaſurement with the chain, ariſes from the extra-length of this laſt, which ſo nearly compenſated for all the irregularities of the ſurface.

The meaſurement with the deal rods being finiſhed, they were compared at 5 h. PM. and found to agree with the ſtandard, the temperature then being 75°.

Expanſion of the Deal Rods.

It has been an opinion generally enough, although, as we have ſeen, erroneouſly received, that very ſtraight-fibred deal was not at all, or but little, affected longitudinally by the humidity of the air. That we might not be led aſtray by truſting to fallacies of this ſort, the ſtandard rod had been provided; which being always cloſely ſhut up in its cheſt, except during the ſhort interim of compariſon, could feel but a ſmall proportion of the effects which the meaſuring rods ſuffered, theſe being conſtantly expoſed to the open air throughout the day, as well as to the moiſture of the night, when lying under the oil-cloth canopy. The ſtandard rod, it is true, could not be accurately compared with the braſs ſcale: for although when conſtructed, braſs pins, forty inches aſunder, had been driven into its ſtem, for the purpoſe of ſuch compariſon, yet theſe had afterwards been diſplaced, or at leaſt the points upon them defaced, by the planing over of the upper ſurface. This circumſtance, which was unattended to when the operations commenced, is now of no conſequence; becauſe, from an experiment hereafter to be mentioned, the lengthening of the ſtandard may be pretty nearly aſcertained. But ſince there are ſome contradictory circumſtances, ſoon to be mentioned, in the operation with the deal rods, which would have made a repetition of it abſolutely neceſſary, if we had not now obtained thoſe of a different kind, ſo very unexceptionable in their nature and mode of application, as, in the preſent caſe, to admit of no competition between the two reſults, and to render it improper on our part ever to have farther recourſe to the firſt; ſo there

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can be little doubt, that deal rods will be univerſally rejected by other countries, in any meaſurements they may have occaſion to make in future.

About the 10th of July, two rods, one of New-England and the other of Riga deal, being meaſured by the fixed points in the great plank in Mr. RAMSDEN's ſhop, and having each two braſs pins driven into them at the diſtance of twenty feet, were laid on the top of the houſe, where they remained until the 26th, the weather, for the greater part of the time, having been very wet. They were then taken down, and being, by means of the long beam compaſſes, compared with the meaſures on the plank, the New-England rod was found to have lengthened 0.031 inch, and the Riga rod 0.041 inch. By which experiment the fact ſeems to be eſtabliſhed, that Riga red wood, notwithſtanding the quantity of turpentine which it contains, is more ſuſceptible of the effects of moiſture than New-England white wood. Mr. RAMSDEN likewiſe finds, that the great plank ſo often mentioned, ſuffers, in ordinary ſummer weather, an alternate expanſion and contraction, amounting at a medium to 0.0041 of an inch every day: that is to ſay, if the diſtance between the twenty-feet braſs points be meaſured from the ſcale, by means of the beam compaſſes, in the evening, it is found to have lengthened next morning 0.0041 of an inch, by the humidity of the intervening night. In the courſe of the following day it contracts again to its former length, and ſo on. Mr. RAMSDEN has often obſerved this alternate change in the deal plank; but it was particularly on the 11th and 12th of Auguſt, that the quantity was actually meaſured. It will readily be underſtood, that any difference of temperature which might have happened in the braſs ſcale, at

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the times of compariſon, was always carefully taken into the account.

Now, from this laſt experiment, it ſeems probable, that we ſhall not be very wide of the truth in ſuppoſing, that the ſtandard deal rod, which lay cloſed up in its cheſt, under the canopy on Hounſlow-Heath, would ſuffer the ſame ſort of alternate expanſion and contraction with the above-mentioned plank; that is to ſay, being of Riga wood, its mean expanſion about the middle of the day would be $\frac{25}{10000}$ of an inch. By this quantity then we muſt augment the actual obſerved expanſion of the meaſuring rods, in order to obtain within certain probable limits (ſince we cannot determine it accurately) the equation for the expanſion; or that ſpace by which the apparent meaſurement, given by the 1370 deal rods, ſhould be augmented in order to obtain the true length of the baſe; or that which would have been given by unalterable rods, of the ſame original length with thoſe of deal, as expreſſed in the following table.

Table

Table of the Expansion of the Deal Rods.

Days.	N° of rods meas.	Hour of comparison.	Temp. of the air.	Observed expansion.	Decimal mean.	Equation for the meas. rods.	Equation for the standard.	Total expansion.
		h. ′	°	In.		In.	In.	In.
July 16	105	4 0 A.M.	48	$\frac{1}{30}$th	0.010	1.050	0.2625	1.3125
		6 0 P.M.	62	0				
17	195	7 0 A.M.	62	$\frac{1}{30}$th	0.010	1.950	0.4875	2.4375
		6 0 P.M.	—	0				
23	240	9 0 A.M.	61	$\frac{1}{30}$	0.021	5.040	0.6000	5.6400
		6 0 P.M.	54	$\frac{1}{67}$				
24	270	7 30 A.M.	61	$\frac{1}{26}$	0.033	8.910	0.6650	9.5750
		11 15 A.M.	66	$\frac{1}{31}$				
		5 45 P.M.	64	$\frac{1}{33}$				
Aug. 2	270	8 30 A.M.	66	$\frac{1}{30}$	0.0125	3.375	0.6650	4.0400
		7 0 P.M.	$67\frac{1}{2}$	0				
3	290	7 0 A.M.	56	$\frac{1}{60}$	0.017	0.493	0.7250	1.2180
		5 0 P.M.	75	0				
Total	1370	— —	— —	— —	— —	20.818	3.405	24.223

N. B. Although the rods were not compared with the standard on the 16th of July, yet the expansion probably was, and therefore has been estimated, at the same rate as it was found on the following day.

By examining the preceding table, it will appear, that the total expansion on the 1370 deal rods, including the small equation for the lengthening of the standard, amounts to 24.223 inches, or 2.02 feet; which being added to the apparent length of the base 27404.31 feet formerly obtained, we shall have, for the hypothenusal length, 27406.33 feet: and from this deducting 0.07 foot, the excess of the hypothenusal above the base line, or the reduction contained in the seventh column of the general table of the base, there will remain 27406.26 for the distance given, by the deal rods, between the

centers of the pipes terminating the baſe, reduced to the level of the loweſt, or that at Hampton Poor-houſe, in the temperature of 63°, being that of the braſs ſcale when the lengths of the deal rods were laid off. All this, however, ſuppoſes three things to be abſolutely certain: firſt, that the expanſion of the rods has been accurately eſtimated; ſecondly, that no error has ariſen from the butting of the rods againſt each other, in order to bring them into contact; and, thirdly, that no miſtake of any kind has been committed in the execution. When we come to give the true length of the baſe, as ultimately aſcertained by means of the glaſs rods, it will appear, that one or more of theſe three have actually taken place; although it is moſt probable, that only the two firſt ſources of error have contributed their ſhare of the total difference between the two reſults. But the diſcuſſion of this point muſt be deferred for the preſent; and I ſhall now finiſh the ſubject of the expanſion of the deal rods, by mentioning two other compariſons of them, which ſerve to ſhew ſtill more obviouſly, how improper they are for very accurate meaſurement!

It has already been remarked, that the laſt week of July was ſo wet as to occaſion a total ſuſpenſion of the operations on Hounſlow-Heath. On the 26th of that month, at 8 h. A.M. the temperature being then 63°, the rods were compared with the ſtandard, and found to exceed it, at a medium, one-fifteenth part of an inch. Now, if we ſuppoſe the whole baſe to have been meaſured with the rods in that ſtate, the difference would have amounted to more than $7\frac{1}{2}$ feet, excluſive of what the ſtandard itſelf might have altered from its original length.

The other compariſon was made at Spring-Grove, in the beginning of September, after our operations on the heath had been finiſhed, and the deal rods with their apparatus depoſited

under the roof of Sir JOSEPH BANKS's Barn. The object here in view was the meaſurement of ſuch a ſpace as the garden would conveniently admit of, when the rods were in their dry or contracted ſtate; and to re-meaſure the ſame ſpace next morning, when the rods, being left out for the purpoſe, had imbibed all the humidity they could from the moiſture of the intervening night. Accordingly, the fourth being a fine dry day, the ſun ſhining bright, and the thermometer about 68°, ſeventeen ſtands were arranged in the long walk, with ſo much nicety in the ſame inclined plane as to appear but like one. The firſt or lowermoſt ſtand had a braſs cock ſcrewed to its top. The two uppermoſt, that is to ſay, the ſixteenth and ſeventeenth, were of the fixed kind, each with a braſs ſlide, and placed only forty-five inches aſunder. The firſt deal rod was made to butt againſt the braſs cock, and the reſt ſucceſſively againſt each other, until fifteen rod lengths were meaſured off, and a fine line drawn on the ſlide marking the extremity of the fifteenth. That rod being removed, forty-five inches, taken from the braſs ſcale, were then laid off backwards from the line on the ſlide of the ſeventeenth to the ſlide of the ſixteenth ſtand, where another fine line was drawn. Thus the ſpace comprehended between this laſt line and the cock on the firſt ſtand, was juſt 300 feet, or fifteen coincident rods. During the night of the 4th, which was very fine, the rods lay on the ſmooth graſs. About ſun-riſing of the 5th there came on a thick fog, which entirely diſpelled about 8 o'clock. At 7 h. A.M. the rods being lifted from the graſs, it was perceived, that the under ſides were perfectly dry, while all the reſt was quite wet with the dew that had fallen. The fourteen ſtands, comprehended between the firſt and ſixteenth, having their diſtances gradually reduced from twenty feet three

inches to twenty feet, the operation of re-meaſurement was then begun, by placing the rods in coincidence with each other (which was now found to be eaſily and accurately effected by a few repeated ſtrokes with a wooden wedge only) until the fifteen rod lengths were meaſured off, and a fine line, correſponding with the ivory on the fifteenth, was drawn on the braſs ſlide. This line was found to be $0.\frac{4\,9\,8}{1\,0\,0\,0}$, or near half an inch beyond that which terminated the 300 feet the preceding evening. Hence it is evident, that the dew imbibed only in one night, or a ſpace of time not exceeding fourteen hours, occaſioned ſuch an expanſion in the deal rods, as in the whole baſe would have amounted to 45.484 inches.

It is ſufficiently obvious, that this laſt mentioned experiment was more accurate, in the proportion of about fifteen to one, than any compariſon we could at that time have made with the ſtandard. But ſince immediately after it was finiſhed, the ſun ſhone out very bright, it is by no means certain, how ſoon the rods would again have contracted to their former length, or near it, had they been expoſed to his rays. Repeated compariſons for aſcertaining facts of this ſort, at very ſhort interims, are abſolutely incompatible with the nature of ſuch tedious and troubleſome operations as the meaſurement of long baſes: and here, indeed, lies the great objection to the uſe of deal rods, that at no time can we be certain how ſoon, after a compariſon has been made, they may alter their length in a proportion, and ſometimes too even in a ſenſe, different from what was expected.

Deſcription

Deſcription of the Glaſs Rods, ultimately made uſe of to determine the length of the Baſe. Tab. XIX.

It has been already mentioned, that the week of rainy weather in the end of July was employed in providing the glaſs tubes, and in concerting matters with Mr. RAMSDEN, relative to their conſtruction as meaſuring rods. Notwithſtanding their great length, they were found to be ſo ſtraight that, when laid on a table, the eye, placed at one end looking through them, could ſee any ſmall object in the axis of the bore at the other end.

The nature and conſtruction of the glaſs rods, whereof three were finiſhed for the operation, will be beſt conceived by conſidering, with care and attention, the plans and elevations of them, in whole or in part, to different ſcales in tab. XIX.; where likewiſe may be ſeen, plans and ſections of the ends of the tubes, in their real dimenſions, for the better underſtanding the ſeveral parts of the apparatus placed therein.

The caſe containing the tube, and which ſerves to keep it from bending in its original ſtraight poſition, is every where of the depth of eight inches, of the ſame width in the middle, and tapers from thence, in a curvilinear manner, towards each end, where it is only two inches and a quarter broad. It is made of clean white deal, the two ſides being half an inch, and the top and bottom three-eighths in thickneſs. Theſe laſt are placed in grooves fitted to receive them, about half an inch from the upper and lower edges of the ſides, which bending eaſily, and applying cloſely, are then firmly faſtened by two rows of wood ſcrews on each ſide, to the top and bottom

reſpectively. Thus, the depth of the ſides in one ſenſe, and the ſpring which they have by bending in the other, act as truſſes, prevent the caſe from warping, and render it ſufficiently ſtrong, although at the ſame time, conſidering its great length, very light.

The plan of the middle rod repreſents the caſe with the top off, that the tube may be ſeen placed therein: the right and left-hand rods have the tops on, whereby may be ſeen the oval opening in the middle of each, ſhut by a mahogany lid; and alſo the poſitions of the two thermometers, with tubes bent at right-angles, ſo as to place the ball about two inches downwards within the caſe, for the better aſcertaining the temperature of the glaſs, as will eaſily be conceived, by conſidering the repreſentation of the tube and ball in the ſection acroſs the middle of the rod.

It is to be obſerved, that the middle of the tube is made faſt to the middle of the caſe in the following manner. Firſt, around the middle of the tube, a quantity of pack-thread, immerſed in liquid glue, was wound by ſeveral returns on itſelf, for the ſpace of about two inches in length; and upon this maſs of pack-thread, while the glue was warm, a ſtrong mahogany collar was forced; whereby the three ſubſtances became ſo perfectly united to each other, that they might be conſidered as one only. Acroſs the bottom of the caſe in the inſide, three mahogany braces or girders, one in the middle, and one half-way between it and each end, are faſtened, by means of ſcrews, to the bottom and ſides. Theſe riſe about $1\frac{1}{2}$ inch above the bottom, ſo as to place the axis of the tube, when in uſe, about $2\frac{1}{2}$ inches above the ſurface of the ſtands on which it reſts. The end-pieces of the caſe are likewiſe of mahogany, about $1\frac{1}{4}$ inch thick. Each conſiſts of two parts, a lower and an

upper.

upper. In the lower parts, as well as in the croſs braces, there are ſemi-circular cavities lined with broad-cloth, fitted to receive the diameter of the tube, which reſts in them, and is conſequently ſupported at five different points. The upper end-pieces, having likewiſe ſemi-circular cavities fitted to embrace the upper part of the tube, ſlip down upon it, when it has been, by repeated trials, brought to its true poſition; that is to ſay, the axis of the bore into the ſame ſtraight line, the caſe being all the while ſupported by its extremities on two ſtands only, in the manner in which the rods are applied in actual meaſurement. The braces within the caſe have alſo their upper pieces, which, in like manner, apply cloſely to the tube, and are fixed to the lower ones by means of ſcrews. The whole together ſerve only as ſtays to keep the tube in its true place from ſhaking; but without binding it however too cloſely. Laſtly, the mahogany collar glued to the pack-thread on the middle of the tube, being ſtrongly fixed by four ſcrews to the middle brace, as may be ſeen in the ſection, is that by which the tube is kept perfectly immoveable with reſpect to the middle of the caſe; while it is unconfined longitudinally in the cavities lined with broad-cloth every where elſe.

Both ends of the tube are ground perfectly ſmooth, and truly at right-angles to the axis of the bore. That end, which in meaſuring uſually lies towards the left-hand (ſince moſt people will work the ſcrew with the right) projects about ſeven-tenths of an inch without the caſe, and is called the fixed end, becauſe the apparatus belonging to it is fixed. The other end towards the right-hand projects about nine-tenths of an inch, and, having a moveable apparatus, is called the moveable end.

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The fixed apparatus conſiſts of a cork about three inches in length, made of the very beſt material, and ſo nicely fitted to the bore as juſt to admit of being forced into without burſting it. In the middle of the cork a cylindrical braſs tube is placed, whoſe ſides are thin, the inward end thick, and the outward end open. It receives a ſteel pin, whoſe inward end being formed into a ſcrew, is thereby fixed into the thick metal of the tube. The ſteel pin carries outwardly a button and neck of bell-metal. The neck fits ſo very cloſely the open end of the braſs tube as to prevent any ſhake there; at the ſame time that the inſide of the button applies very juſtly to the ground end of the glaſs tube, to which the outward ſurface (being a true plane) is exactly parallel.

The moveable apparatus conſiſts, like the other, of a cork and braſs tube of the ſame length. Before the inſertion of this cork, an oblong piece ſeven-tenths of an inch long, and two-tenths broad, was cut from it, in that part of its cylinder anſwering to the upper part of the outward end of the glaſs tube, on the inward ſurface of which, about half an inch from the end, a fine line had been previouſly cut by a diamond point. The braſs tube in this cork contains within it a looſe ſteel worm, or helical ſpring, ſomething leſs than the interior diameter of the tube. Along the cavity formed by the ſpiral, there paſſes a ſteel pin, like that in the fixed end; but it is longer, and has no ſcrew at the inward end, that being nicely ground, ſo as to fit a circular hole in the inward end of the braſs tube, while a triangular bell-metal neck fits one of that figure in the outward end. Thus the pin moves freely backwards or forwards without any ſhake, and preſſes upon the ſteel ſpring, by means of a circular braſs collar, placed for the purpoſe, at the inward end of the neck; while the outward

end is attached to a bell-metal button. The outward ſurface of this moveable button is ſpherical, deſcribed on a radius of about two inches; while the inward ſurface, like that at the fixed end, would apply cloſely to the ground end of the glaſs tube, but ſhould not be puſhed ſo far forward as to touch it. A circle and narrow ſlide, cut from a ſolid cylinder of ivory, fitted originally to enter eaſily the glaſs tube, is attached to the inſide of the button by ſmall ſcrews, and permits the neck to paſs through a hole made on purpoſe in the circle. The ſlide is about eight-tenths of an inch long, and has a fine interſection cut upon it near the inward end, made black to render it more conſpicuous. Thus, two rods being brought into contact, and the fixed button of one being preſſed againſt the moveable button of the other, the interſection is thereby puſhed forwards until it coincides with the diamond line on the interior ſurface of the tube; whoſe length is ſo adjuſted, as that, when the coincidence is perfect, the diſtance between the plane ſurface of one button, and the ſpherical ſurface of the other, is exactly twenty feet. The left-hand ſide of the plate repreſents the relative poſitions of the extremities of the firſt and ſecond rods, when the ivory is in coincidence with the diamond line. And the right-hand ſide ſhews the relative ſituations of the extremities of the ſecond and third rods, before the ivory is brought to coincidence with the diamond line, the ſlide being then puſhed out by the action of the ſpiral ſpring within the cork.

Every rod has four wheels, two at each end. They are two inches in diameter, and connected by a common ſteel axis, which riſes and falls in a vacuity prepared for its admiſſion in the mahogany end-pieces, the under part of which vacuity is afterwards filled up.

A braſs

A braſs ſtrap or bridle, about eight-tenths of an inch broad, paſſes over the top of the caſe, and deſcending down each ſide, bends outwards, ſo as to form a projection for the reception of the wheels, whoſe pivots turn in, but near to the lower end of the bridle, which is kept in its place by means of the two ſide ſcrews working in grooves, and the milled-headed ſcrew at top. This laſt ſerves likewiſe to raiſe or depreſs the wheels at pleaſure.

Each rod has two croſs feet, placed immediately behind their reſpective pair of wheels, extending outwards about $4\frac{3}{4}$ inches from the center on each ſide. Under their outward extremities, ſmall pieces of hardened ſteel, formed into the teeth of a file, are fixed by means of ſcrews. When the firſt rod has been laid in its true place, by unſcrewing the milled heads, the wheels are ſuffered to riſe; whereby the whole weight is removed from them, and thrown upon the teeth of the files, which then indent themſelves into the ſurface of the ſtand, and become as it were united to it. But when the fixed button of the ſecond rod is brought to preſs againſt the moveable button of the firſt, the weight being then thrown upon the wheels by ſcrewing the milled heads at top, the rod is eaſily moved on by the following apparatus.

The three rods are numbered, as were thoſe of deal, 1.2; 3.4; 5.6. On the firſt or odd end of each rod 1. 3. and 5. there ſtands a braſs fork, about two inches high, fixed by four ſcrews and an oblong plate to the top of the caſe. On the ſecond, or even end of each, 2. 4. and 6. there ſtands a braſs pillar of the ſame height with the fork, likewiſe fixed to the top of the caſe by four ſcrews and a circular plate. Two ſteels rods or hooks were indifferently uſed for bringing up the moveable rod (the weight then lying on the wheels) into its true place. They

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are both repreſented in the plate, and only differ from each other in the ſhape of the braſs milled-headed nuts that work upon the ſcrew, of about $2\frac{1}{4}$ inches in length, into which the right-hand end of each hook is formed. Thus, while the nut enters very freely into, and reſts upon, the fork, the left-hand end of the hook has a circular hole in it, whereby it ſlips eaſily off and on of the braſs pillar. By referring to the plate, it will appear ſufficiently obvious, from the nature of the nut on the left-hand hook, that it could only move the rod on to coincidence, and could not bring it back again, if the buſineſs happened at any time to be overdone; in which caſe it was neceſſary to move the rod a little backwards by the hand, and then to work anew with the nut, until the coincidence was accurate: whereas the nut on the right-hand hook, having two ſhoulders, could either puſh or pull the rod forwards or backwards: and although this appeared to be an advantage, yet it was found from experience, that it rather bound the hook too much, and occaſioned a kind of ſpring in the parts, which ſometimes diſturbed the coincidence on the removal of the hook; wherefore it was often applied, like the other, by placing the ſcrew itſelf in the fork, and working with both ſhoulders of the nut behind it.

The poſitions of the thermometers, and mahogany oval lid on the top of the caſe, have already been mentioned. This laſt, being unlocked and removed, permits the caſe to be looked into, or the hand to be admitted, in order to be certain that the faſtenings remain ſafe and entire in the inſide. Braſs caps, with the reſpective number of the rods engraved on them, are likewiſe ſcrewed on the male-ſcrews in the ends of the caſe, through which the extremities of the tubes project, to preſerve them from accidents when not in uſe. And, laſtly, to

ſtrengthen the caſes, but more particularly to prevent them from being rent when long expoſed to the ſun's rays in the field, the ſides are covered with brown linen laid on very ſmoothly, and carefully glued with thin glue, uſed as a ſtronger kind of paſte, to which it may yet be neceſſary to add a coat of oil paint.

Each of the glaſs rods, completed in the manner above-mentioned, weighs about ſixty-one pounds. Their lengths were aſcertained by means of new braſs points placed in the great plank, the ſpaces of forty inches being laid off, with the utmoſt care, from the braſs ſcale, when the temperature of all had remained for the greater part of two days (Auguſt 15th and 16th) at or very near 68°. For this purpoſe two braſs rectangular cocks, whoſe alternate ſurfaces had been previouſly ground together, were placed upon the plank, ſo as to biſect the extreme dots; in which ſituation they preſented to each other ſurfaces that were truly parallel. The rods being then ſeverally placed between the cocks * (or, as was found to be a better method,

* The firſt of theſe cocks, or that to which the fixed button was applied, had a hole in it exactly of the height of the center of the button, and large enough to permit the point of the micrometer ſcrew to paſs through it, the ſaid ſcrew being fixed on the farther ſide, or beyond the cock. Thus, while the temperature continued accurately at 68°, the fixed button, or any other plane ſurface, being brought up to the hole in the cock, and the micrometer point ſcrewed ſo far as juſt to touch it, the coincidence continuing in the interim perfect, the exact diſtance of twenty feet was obtained between the point of the ſcrew and the ſecond cock; at which time the diviſion anſwering to the index on the head of the micrometer was carefully noted. This being done, the cock with the hole was removed from the plank, and the rods were ſeverally adjuſted by being placed between the point of the ſcrew and the ſecond cock. This ſubſtitution of the micrometer point, inſtead of the firſt cock, was found neceſſary; becauſe, during the operation of adjuſtment, the temperature would ſometimes change a degree, generally

method, between the point of a micrometer ſcrew, ſupplying the place of the firſt cock, and the ſecond) the ivory interſection was at firſt neceſſarily carried beyond the diamond line, ſo as to make the intermediate ſpace leſs than it ſhould be, until by the gradual grinding down of the moveable bell-metal button, it was enlarged to twenty feet, as then ſhewn by the accurate coincidence of the interſection with the diamond line.

It was by theſe diſtances in the great plank, prolonged to twenty-five feet, that the new length of the ſteel chain was now ſettled, ſo as to obtain the full one hundred feet at four meaſurements. At this time too, braſs points were introduced into the chain at every twenty-five feet, whereby its extent may be compared on any future occaſion; but the temperature had now fallen to 66°½.

Diſpoſition of the Stands for the double meaſurement with the Chain and Glaſs Rods; deſcription of the apparatus then applied to the ends of the Chain; and ultimate continuation of the meaſurement with the Glaſs Rods alone. Tab. XVII. and XIX.

From the various circumſtances already mentioned, in the courſe of this tedious, yet neceſſary recital, it had been for a conſiderable ſpace of time foreſeen, that the reſult given by the meaſurement with the deal rods muſt be entirely rejected,

generally in exceſs, from handling the inſtruments. One degree of alteration, producing a difference of about $\frac{1}{10000}$th part of an inch in the twenty feet, was very eaſily and accurately allowed for by ſuch a micrometer as this, which ſhewed the coincidence of the ivory interſection with the diamond line to be more or leſs perfect, when the head of the ſcrew was moved two diviſions, that is to ſay, $\frac{2}{10000}$ths or $\frac{1}{5000}$th part of an inch.

and that by the glaſs rods adhered to as every way deſerving of the preference; becauſe of the obvious impropriety there would be, in taking a mean between one indiſputably good and another leſs perfect, however ſmall or trifling in reality the difference of the two might ultimately be found, on a minute and ſcrupulous compariſon.

In order, therefore, to avoid any repetition of the operation with the glaſs rods, and at the ſame time to give ſomething like a fair trial to the chain, it was propoſed, that a double meaſurement ſhould be carried on with both at once; that is to ſay, that the number of ſtands, and ſeveral other parts of the apparatus, ſhould be ſo far augmented, as to admit the chain to be placed twice in advance, and then the rods to follow in ſucceſſion on the ſame ſtands. Accordingly, the various articles having been ſent to the north-weſt end of the baſe on the evening of the 17th of Auguſt, the operation of the double meaſurement commenced next morning the 18th.

By referring to tab. XVII. it will be ſeen, that ſeventeen ſtands were neceſſary for ſupporting the chain, the apparatus attached to each end of it, and ten coffers, whereof every five made about ninety-eight feet, in order that one length of the chain being meaſured off in the firſt five, it might be drawn forward into the laſt five, and ſo on. Theſe ſeventeen ſtands were diſpoſed of in three groups of three each, and four intermediate, between the central and extreme groups. The middle or ſlide ſtand of each group (ſo diſtinguiſhed becauſe ſome of them had braſs ſlides on their tops) ſupported the handle of the chain, and of courſe received the traces made at the feather-edged pieces of braſs, terminating the beginning and ending of the hundred feet. Thus, there were in all ſix ſtands, intermediate to thoſe in the center of each group that ſupported

the ninety-eight feet of coffering, which was kept ſo much ſhort of the hundred feet, that its extreme parts might not reſt upon, or even touch, the central ſtands. To that on the left of the center was attached the apparatus for the firſt or zero end of the chain; and to that on the right of the center was attached the apparatus for the laſt end of the chain. When the ſecond chain length had been meaſured off, the firſt and ſixth of the coffer ſtands of the firſt chain were moved forward to prepare for the third chain; and the four remaining coffer ſtands were raiſed, until their ſurfaces came into the ſame plane with the ſlide ſtands, for the reception of the glaſs rods. The ſpace by which theſe ſtands were raiſed was about three inches; for ſo much higher was the ſurface of the interſole or flooring of the coffers than the ſtands which ſupported them.

The apparatus attached to the firſt end of the chain, or that which ſerved to pull it back to the point of commencement, while a weight continued ſuſpended at the farther end, conſiſts of two parts, as may be ſeen by referring to the left-hand ſide of tab. XVII. Firſt, a ſmall wooden frame, fitted to ſlip on to the top of any one of the ordinary ſtands, placed immediately to the left of that which ſupports the handle. Secondly, a flat ſteel rod, about two feet in length, wherein a number of holes are piereed, about an inch aſunder, for the reception of a ſteel pin placed in one of the holes, as beſt ſuits the diſtance of the ſtand from the handle. That end of the ſteel rod neareſt to the end of the chain is formed into a ſcrew about four inches in length, and it receives upon it a forked hook fitted to lay hold of the ſtraight part of the handle of the chain. Within the forked hook there works a ſtrong milled-headed braſs nut, which acting upon the bottom of the fork, the chain is thereby pulled back, until the wire ſuſpending the plummet

from

from the dart on the feather-edge coincides with the point of commencement on the ground underneath; for which purpoſe there is a hole in the top of the ſtand through which the wire paſſes. The apparatus ſtand, thus ſerving to pull back the chain, was commonly loaded with double weights, placed on the two hindermoſt legs.

The apparatus for the laſt end of the chain conſiſts, like the former, of a ſmall wooden frame that can be readily ſlipped upon any of the common ſtands, as may be ſeen by referring to the right-hand ſide of tab. XVII. This frame carries a pulley, over which a rope paſſes having fourteen pounds weight ſuſpended at one end of it, while a forked iron hook at the other end lays hold of the ſtraight part of the braſs handle. By means of theſe two apparatuſes the chain is always kept to the ſame degree of tenſion in its coffers, in each of which a thermometer was placed to indicate the temperature; the whole being covered up from the direct rays of the ſun by a narrow piece of linen cloth, ſtretched along it from one end to the other.

Each coffer conſiſted of three boards about half an inch thick. The ſides were about five inches deep, nailed at the middle to an interſole bottom of four inches, in ſuch manner as to be repreſented in ſection by the letter H. They were ill made, being by their parallelogram ſhape apt to warp, which might have been prevented by giving them the figure of the caſes of the glaſs rods, that is to ſay, making them wide in the middle and narrow at each end.

We are now to proceed to give ſome account of the double meaſurement with the chain and glaſs rods; wherein it muſt be remembered, as alſo in continuing the operation with the glaſs rods alone, that in referring to the map for the daily progreſs in the work, we are going from the forty-ſixth towards the firſt ſtation;

ſtation; and in having recourſe to the general table of the baſe, for altitude, temperature, or correction for expanſion, we are aſcending from the bottom towards the top, contrarily to the order in which the operation with the deal rods was conducted.

On the morning of the 18th of Auguſt, the ſtands with the various parts of the apparatus being placed in the manner juſt now deſcribed, the operation was begun by bringing the firſt end of the chain to coincide with the interſection on the tripod, anſwering to the end of the 1370th deal rod, and 4.31 feet diſtant from the center of the pipe terminating the north-weſt extremity of the baſe. The chain being ſtretched along its five coffers by the fourteen pounds weight ſuſpended over the pulley at the farther end, and the temperatures of the five thermometers being regiſtered in a book kept for that purpoſe, a fine trace was made on a piece of card faſtened under the feather-edge at the farther handle, denoting the end of the firſt hundred feet. The chain being then moved on into the next five coffers, thoſe that had been thus vacated were carried forward to prepare for the third chain length, and thereby permit the firſt ſet of ſtands to be elevated for the reception of the glaſs rods; and ſo in ſucceſſion with the others.

In this manner we proceeded, and in the courſe of the day were only able to meaſure the length of ten chains, or 1000 feet, being the forty-ſixth and forty-fifth hypothenuſes of the baſe, the firſt of 400 and the laſt of 600 feet. Being arrived at this point it was found, that the fine line on the braſs ſlide, marking the extremity of the tenth chain, fell ſhort of another fine line on the ſame ſlide, denoting the end of the fiftieth glaſs rod, juſt two-tenths of an inch. Now it will appear hereafter, when we come to ſhew, by the experiments with the pyrometer, what the real contractions of the chain and

glaſs

glaſs rods were, for the degrees of difference of temperature * below that in which their reſpective lengths were laid off, that this ſmall apparent difference of two-tenths of an inch, between the two modes of meaſuring the thouſand feet, ſhould have been 0.17938 in. to have made the two reſults exactly agree, which is a real difference of only 0.02062 of an inch. Suppoſing then every thouſand feet of the baſe to have been meaſured by the chain with the ſame attention, and conſequently with the ſame, or nearly the ſame ſucceſs (and there ſurely cannot be any reaſon to doubt of the practicability) we ſhall have 27.404×0.02062 in. $= 0.565$ in. or a defect of ſomething more than half an inch on the whole length of the baſe.

* When the length of the chain was laid off, the heat was 66°½, and that of the glaſs rods 68°. They will, therefore, only agree with each other accurately in theſe reſpective temperatures. The mean of twenty thermometers for the four chain lengths of the forty-ſixth hypothenuſe gave a heat of 61°.6; and for the ſix chain lengths of the forty-fifth, the mean of thirty thermometers gave 59°.75. The temperature of the 400 feet of glaſs by the mean of forty thermometers was 65°.3; and of the 600 feet, by the mean of ſixty thermometers, it was 60°.8. Now, from theſe data, and the expanſions of ſteel and glaſs, as determined by the pyrometer, the computation will ſtand as follows:

				In.	In.	In.	
Steel	400	66.5° − 61.6°	= 4.9	× 0.03052	= 0.14955	= 0.45856	contract. of 1000 feet.
	600	66.5° − 59.75°	= 6.75	× 0.04578	= 0.30901		
Glaſs	400	68.0° − 65.3°	= 2.7	× 0.02068	= 0.05584	= 0.27918	contract. of 1000 feet.
	600	68.0° − 60.8°	= 7.2	× 0.03102	= 0.22334		

The 1000 feet of ſteel ſhould have contracted more than the 1000 feet of glaſs, - - -	= 0.17938
But the difference was found to be - -	= 0.20000
Therefore the error of the chain in defect was -	0.02062 × 27.404 = 0.565 in. or little more than half an inch on the whole baſe.

So

So nice an agreement between two reſults, with inſtruments ſo very different, could not fail to be conſidered as aſtoniſhing; and as it rarely happens, that the graduation of thermometers will ſo nearly correſpond with each other, as not to occaſion a much greater error, all were very deſirous that it could have been farther confirmed by continuing the operation in the ſame way through a more conſiderable proportion of the whole length. But beſides the tedious nature of the double meaſurement, owing to the multiplicity of ſtands, platforms, coffers, and other articles, that were now ſucceſſively to be moved forward, and for which purpoſe it had been found neceſſary to re-inforce the party of ſoldiers with ſix additional men; the operation had already trained out to a much more conſiderable length than had been expected; the ſummer was now far advanced, and the continuance of good weather uncertain; the coffers likewiſe for the chain, having been conſtructed in a hurry, were found to be defective: in ſhort, all theſe reaſons contributed to induce us to give up, for the preſent, any farther experiment with the chain, and to proceed with the glaſs rods alone in the completion of the meaſurement.

Accordingly, on Thurſday the 19th of Auguſt, the operation with the glaſs rods was continued for the five hypothenuſes, from the forty-fourth to the fortieth incluſive. It will be remembered, that in proceeding with the deal rods, double pickets had been placed in the ground, at the middle of the forty-firſt hypothenuſe, or that point which terminated the 1215th rod, reckoning from the ſouth-eaſt, or the 155th from the north-weſt end of the baſe. Now, in returning to this point with the glaſs rods, the extremity of the 155th fell ſhort of the ſilk thread ſtretched from picket to picket, juſt one-tenth of an inch. The expanſion of the braſs ſtandard ſcale, and

that of glaſs being taken into the account, it appears, that the ſmall expanſion * of the deal rods from the humidity of the air, muſt, at this point, have exceeded what it was eſtimated at in the general table by 0.931 of an inch, ſuppoſing no error of any kind whatever to have ariſen in the execution, from bringing the rods into contact, or otherwiſe.

On Saturday the 21ſt of Auguſt, the meaſurement was reſumed at the thirty-ninth ſtation, and continued for five hypothenuſes to the thirty-fifth incluſive.

This day, about noon, HIS MAJESTY deigned to honour the operation by HIS preſence, for the ſpace of two hours, entering very minutely into the mode of conducting it, which met with HIS gracious approbation.

On Monday the 23d, the menſuration was farther continued for five hypothenuſes, that is, to the thirtieth incluſive.

On Tueſday the 24th, we proceeded with the meaſurement for the ſpace of ſeven hypothenuſes, finiſhing the buſineſs of the day at the twenty-ſecond ſtation.

		In.
* 155 deal rods = 3100 feet	+	0.383 for 1d exceſs of temperature of the braſs ſcale from 62° to 63°.
	+	0.651 proportionable part of the eſtimated expanſion from humidity.
	+	1.034 equation of the deal rods on 3100 feet.
155 glaſs rods = 3100 feet	+	2.301 for 6° exceſs of the heat of the braſs ſcale from 62° to 68°.
	−	0.436 obſerved contraction of the glaſs from the 11th and 12th columns of the table.
	+	0.100 by which the 155th rod fell ſhort of the thread.
	+	1.965 equation of the glaſs rods on 3100 feet.
0.931		Difference of the two equations, under-rated in the expanſion of the deal rods.

It

It will be remembered, that in carrying on the operation with the deal rods, double pickets were left in the ground at the twenty-ſeventh ſtation, anſwering to the extremity of the 810th rod from the firſt, or the 560th from the laſt end of the baſe. Now, on arrival at this point, the 560th glaſs rod overſhot the ſilk thread, ſtretched from one picket to the other, 2.525 inches. Here again we find, that the lengthening * of the deal rods from the moiſture of the atmoſphere differs but little from what it has been eſtimated at by compariſon with the ſtandard, being over-rated only two-tenths of an inch on the 560 rods. In this day's operation, in paſſing the bridge laid over the old river, the meaſurement, inſtead of being made in the hypothenuſal, was carried on in the level line, for the ſpace of twenty rods, namely, fifteen rods of the twenty-ſeventh, and five of the twenty-ſixth hypothenuſe; which occaſions the alteration in the reduction of theſe two ſpaces, marked with aſteriſks in the general table.

As ſome trouble had been found to attend the croſſing of the great road, in the firſt meaſurement, owing to the number of carriages that were continually paſſing, the depth of

* 560 deal rods = 11200 ft.

In.	
+1.390	for 1° exceſs of heat of the braſs ſcale from 62° to 63°.
+5.258	eſtimated expanſion from moiſture,
+6.648	equation of the 560 deal rods.

560 glaſs rods = 11200 ft.

+8.343	for 6° exceſs of heat of the braſs ſcale from 62° to 68°.
+1.821	obſerved expanſion of glaſs } from columns 11th
−1.191	obſerved contraction of ditto } and 12th.
−2.525	over-ſhot the ſilk-thread.
+6.448	equation of the 560 glaſs rods.

0.200 Difference over-rated in the expanſion of the 560 deal rods.

the ditches, and height of the banks of the old Roman way; therefore treſſels, ſuited for the purpoſe, had been now prepared: and leſt any accident might have happened in conducting this part of the operation, ſo as to oblige us to a repetition, double pickets were placed in the uſual manner in the ground two rod lengths from the twenty-ſixth ſtation, to which we could have referred, without going back as far as the tripod left at the twenty-ninth ſtation, the point from which we had departed in the morning.

Bad weather prevented any progreſs being made on the 25th; and, on the 26th, all that could be done was to meaſure the twenty-ſecond and twenty-firſt hypothenuſes.

On Friday the 27th, the work went on more expeditiouſly, having in the courſe of that day meaſured ſix hypothenuſes, and placed the tripod at the fourteenth ſtation.

On Saturday the 28th, eight hypothenuſes were meaſured, and the tripod was placed at the ſixth ſtation. In this day's operation, being arrived near the bridge laid over Wolſey River, double pickets were placed in the ground in the point anſwering to the extremity of the 1172d rod, reckoning from the north-weſt, or the 198th rod from the ſouth-eaſt end of the baſe, that we might recur to them in caſe of accident; and the eighteen rod lengths, between this point and the ſixth ſtation, were meaſured on the level, inſtead of the hypothenuſal line, which required the alteration of the reduction as diſtinguiſhed by the aſteriſk in the general table.

On Monday the 30th of Auguſt, the meaſurement with the glaſs rods was completed*; when the extremity of the 1370th rod

* The gentlemen who were preſent at, and aſſiſting in, the laſt day's operation were Captain Bisset, Mr. Greville, Sir William Hamilton, Mr. Lloyd, and

rod over-ſhot the center of the pipe terminating the baſe towards the ſouth-eaſt by 17.875 inches, or 1.49 foot. Hence, when the ſeveral equations for expanſions are reſpectively taken into the account, we find, that the alteration of the deal rods from the humidity of the air, which, by compariſon with the ſtandard, was apparently moſt conſiderable in the firſt and ſecond ſections of the baſe, has now wholly vaniſhed; that is to ſay, the total amount of it has been over-rated by 20.964 inches*; and this is the contradictory circumſtance that has been formerly alluded to.

I have already ſuggeſted what appear to me to have been the only three poſſible cauſes of this difference, found between the eſtimated and real expanſion of the deal rods; and as we are to abandon that meaſurement entirely, it is of little or no importance now to endeavour to diſcover, were it poſſible, whence it may have ariſen. If any error was actually com-

and Dr. USHER, Profeſſor of Aſtronomy in the College of Dublin. This laſt gentleman was ſo obliging as to obſerve, with the moſt ſcrupulous attention, throughout the whole operation with the glaſs rods, that the coincidence of the ſecond with the firſt remained undiſturbed, while that of the third with the ſecond was completing.

* 1370 deal rods = 27400 ft.

In.	
+ 3.389	for 1° of the braſs ſcale from 62° to 63°.
+ 24.223	eſtimated expanſion from humidity.
+ 27.612	equation of the 1370 deal rods.

1370 glaſs rods = 27400 ft.

+ 20.336	for 6° of the braſs ſcale from 62° to 68°.
+ 5.989	obſerved expanſion of glaſs } from columns 11th
− 1.802	obſerved contraction of ditto } and 12th.
− 17.875	ſpace by which the 1370th rod over-ſhot the pipe.
+ 6.648	equation of the 1370 glaſs rods.

20.964 over-rated in the total expanſion of the deal rods.

mitted,

mitted, which is the leaſt of all probable, it could only have happened at the place of the tripod, by bringing a wrong point of the ſtem over it when the operation was reſumed. But it is well known, how much care and pains were taken to prevent any thing of that ſort. Indeed the hypothenuſal diſtances, as given by the chain, agreed ſo nearly among themſelves, that even a foot or ten inches would have made ſo remarkable a difference in the ſituation of the next picket as could not have paſſed unobſerved. Beſides, in returning with the glaſs rods, after paſſing the Staines Road, the meaſurement was gradually found (without any leap whatever) to over-ſhoot the pickets, and at laſt over-reached the ſouth-eaſt pipe by 17.875 inches. I am therefore inclined to believe, that the difference ariſes partly from what may have been loſt by conſtantly butting one rod againſt the other, whereby the end of the 1370th did not reach ſo near to the north-weſt pipe as it ought to, and would have done, if the rods had been applied to each other by coincident lines. It muſt, however, be confeſſed, that the near agreement between the glaſs and deal rods in the upper part of the heath ſeems not perfectly reconcileable to this ſuppoſition. Nevertheleſs, the deſcent being quickeſt, and the irregularities of the ſurface much more conſiderable in the lower than the upper part, might produce ſome effect in one which did not take place in the other. But the chief part of the difference I take to have proceeded from over-rated expanſion; that is to ſay, the rods, when brought into uſe, contracted ſooner than we imagined, and thereby gave a ſhorter meaſure than what was aſſignable to them from the mean of any two or more compariſons.

The

The laſt day of Auguſt was employed in diſcharging the party, and removing the various parts of the apparatus to Spring-Grove Houſe.

Deſcription of the Microſcopic Pyrometer, made uſe of for determining by experiment the expanſion of the metals concerned in the meaſurement of the Baſe.- Tab. XX.

Having, in the preceding part of this Paper, given a very minute account of the actual operations in the field, that the Public, being thus informed of every circumſtance, might be the better enabled to judge of the accuracy of the reſult, it remains yet to point out, in what manner the equations for the expanſions of the ſtandard ſcale, ſteel chain, and glaſs rods, applied to the apparent meaſurement of the baſe, in ſeveral of the preceding notes, have been obtained by means of experiments with the pyrometer.

It is ſufficiently well known, that many years ago, a very ingenious and valuable Member of this Society did publiſh in the Philoſophical Tranſactions (vol. XLVIII. 1754, N° 79.) an account of experiments made with a pyrometer of his invention. No doubt was entertained of the accuracy of the experiments here alluded to; on the contrary, they will be confirmed by the account now to be given of theſe recently made, with which they very nearly agree. But as different pieces of metal of the ſame kind are certainly ſuſceptible of different degrees of expanſion, it was judged beſt, on the preſent occaſion, to put rods to the teſt of thoſe very metals that had been made uſe of in the actual meaſurement of the baſe.

For, ſuppoſing both ſets of experiments to have been made with inſtruments equally perfect, and to have been in other reſpects equally well conducted, this muſt always be conſidered as the moſt unexceptionable method. Beſides, the expanſion of rods of the length of five feet being aſcertained, the unavoidable error of obſervations of this delicate nature, becomes leſſened in proportion to the exceſs of their length above ſhorter rods. In theſe new experiments too, another ſort of pyrometer, invented by Mr. RAMSDEN, has been applied, of ſuch accurate conſtruction that it ſeems not eaſy to improve it.

The microſcopic pyrometer, ſo named becauſe, by means of two microſcopes attached to it, the expanſion is meaſured, conſiſts of a ſtrong deal frame five feet in length, nearly twenty-eight inches broad, and about forty-two inches in height. The elevation of the eye-piece ſide, or that which preſents itſelf to the obſerver, and alſo of the micrometer end, or that which is towards his right-hand, as well as the general plan of the top, are repreſented by a ſcale of one inch to a foot, or one-twelfth part of the real dimenſions, in tab. XX. where likewiſe may be ſeen the angular view of the fixed end, together with plans, ſections, and elevations, of ſeveral of the principal parts, done to larger ſcales. From theſe, it is hoped, the conſtruction of the machine will be eaſily underſtood, without entering into a minute deſcription of the almoſt numberleſs ſmaller parts whereof it is compoſed.

On the top of the frame, two deal troughs, upwards of five feet in length, are firmly ſcrewed. That towards the obſerver overhangs the frame ſomething more than an inch: that on the farther ſide is even with the back part. Each of theſe troughs, which are about three inches ſquare in the inſide, contains a caſt-iron ſtandard priſm, whoſe ſides are $1\frac{1}{4}$ inch.

The

The manner in which the prisms are fastened to the bottoms of their respective troughs, and the nature of the apparatuses they carry on their extremities, will be readily conceived, by referring to the particular plans and elevations of them, comprehended in the group of eight small figures towards the right-hand of the general plan. Four of these appertain to the left-hand or fixed microscope; and the other four to the right-hand or micrometer microscope, so distinguished because it has a micrometer attached to it. By means of the brass collars which embrace the prisms, their left-hand or fixed ends are screwed down extremely fast to the brass pieces whereon they rest, so as to be perfectly immoveable there with regard to their troughs; whereas their right-hand ends are kept easy, yet without shake, in their collars, that they may contract or lengthen freely as the temperature may require, without occasioning any strain upon the parts. The prism in the nearest trough may be called the eye-piece prism, because it carries the eye-pieces of the microscopes; and that in the farther trough, the mark prism, because it carries the marks or cross wires at which the microscopes respectively point. The troughs are covered with pitch in the inside, to make them hold water; and each has a cock in the left-hand end for discharging it.

Between the two deal troughs, one of copper, as a boiler, is placed, somewhat shorter than the former, but still upwards of five feet in length. It is about $2\frac{3}{4}$ inches broad, and $3\frac{1}{2}$ in depth. The center of the boiler, or rather the center of the object lens which stands in it, as we shall have occasion soon to point out, is distant from the cross wires of the mark 5.81 inches; and from the wires of the micrometer attached to the corresponding eye-piece 20.33 inches. The boiler rests on five small rollers, one being fixed to each end of the frame, and

the other three to the braces which run acroſs it. This copper trough has likewiſe a cock in the left-hand end; and in the general plan a caſt iron priſm is repreſented in it; but this laſt carries no apparatus, as thoſe in the wooden troughs do, being exactly of the length of five feet, and only placed there as one of the rods whoſe expanſion was tried, and to ſhew that the machine was capable of receiving a rod of that weight and magnitude.

By referring to the general plan it will be ſeen, that twelve lamps are made uſe of to bring the water in the copper to boil. They ſtand on four ſhelves, three in each compartment formed by the croſs braces of the frame. They can readily be puſhed forwards or drawn backwards, and when actually in uſe, their handles are only ſeen, projecting from under the copper. It was found, by burning oil in the lamps, the heat of the water could not be raiſed above 209° or 210°; but with ſpirits of wine it was brought into violent ebullition. The plan of the frame likewiſe ſhews, that the tubes of the microſcopes are ſub-divided into ſeveral diſtinct parts; and that one of theſe parts is attached by a collar to a mahogany priſm, which reaches from one end to the other. But the uſe of theſe contrivances it will be beſt to defer ſpeaking of, till after having deſcribed the apparatuſes that are placed within the copper boiler.

At the bottom of the plate the boiler is repreſented, both in plan and longitudinal ſection, to a ſcale of one-fourth part of its real dimenſions. It contains within it two braſs ſlides, the one long and the other ſhort; which, from the braces that bind the cheeks together, very much reſemble the form of a ladder. The long ſlide, whoſe cheeks are $1\frac{3}{4}$ inch deep, reaches almoſt the whole length of the copper, although every where unconnected with it except at the points A and B. At

the firſt of theſe, two ſtrong pieces of braſs, fixed to the cheeks, and notched underneath, embrace the ends of a braſs cylindrical bar faſtened to the bottom. At the laſt, the cheeks of the ſlide reſt on a roller. Whence it follows, that the copper and ſlide remain immoveable with regard to each other at A; but from thence, towards either end, they have full liberty to change place; that is to ſay, to expand by heat, or contract by cold, in any proportion their different natures may require. The left-hand end of the ſlide is ſhut up by a ſtrong perpendicular piece of braſs, connected with the two ſide rings which ſupport the object lens of the fixed microſcope, whoſe center correſponds accurately with its inward face. This piece being firmly ſcrewed to the cheeks of the ſlide, and counter-arched outwardly, forms a ſtrong butt for the fixed end of the expanding rod (ſuppoſed here to be the ſteel bar) to act againſt. Within the right-hand end of the long ſlide, reſts a ſhort one of about 14½ inches in length, whoſe cheeks are 1¼ inch deep. Its outward end, at C, reſts on the cylindrical ſurface of the laſt brace of the long ſlide, fitted purpoſely to receive it; while a narrow longitudinal bar fixed in its inward end, at DE in the ſection, moves freely in the notch of a bridge F, framed for it in the long ſlide. The outward end of this ſhort ſlide is ſhut up in a ſimilar manner with the oppoſite end of the long one.

This end-piece is alſo connected with the two ſide rings which ſupport the tube containing the object lens of the micrometer microſcope, whoſe center is perpendicularly over its inward face, and being fortified outwardly by an edge bar, it forms a butt for the expanding end of the rod that is in experiment to puſh againſt. By attending to the plate it will be perceived, that to this end of the boiler a braſs tube (R) is fixed, which contains within it a braſs rod, ſurrounded by a

helical ſteel ſpring; which acting upon a broad ſhoulder of the rod prepared for the purpoſe, thereby preſſes its inward end, which enters the boiler, againſt the perpendicular ſurface of the end-piece of the ſhort ſlide. Thus, the farther end of the rod in experiment, ſuppoſed now to be in its contracted ſtate, is conſtantly made to bear againſt the ſurface that is under the fixed microſcope. But on the application of heat, the irreſiſtible force of expanſion in the rod obliges the ſpring to give way; the ſhort ſlide changes its place, and with it the object lens of the micrometer microſcope moves on a ſpace proportionable to the degree of heat that is applied; and it is this diſtance, meaſured by means of the micrometer, as hereafter will be ſhewn, that determines the quantity of expanſion, or the ſpace by which the rod has lengthened. From the plate it will be further obſerved, that the rod in experiment reſts on the ſurfaces of three rollers, about an inch in diameter; and by means of three pair of milled-headed nuts $1\frac{1}{2}$ inch in diameter, which move on axes that are formed into ſcrews, until they almoſt touch the ſides of the rod, this is kept in its true centrical poſition, whatever may be its form or lateral dimenſions.

The microſcope towards the left-hand has been denominated fixed, becauſe it correſponds with the firſt or fixed end of the rod in experiment, and never changes its place while theſe are of the length of five feet. But it appearing to be of conſequence, that the expanſion of the ſtandard braſs ſcale, which is not quite forty-three inches long, ſhould be determined, the pyrometer has therefore been adapted for the reception of any rods leſs than five feet, whereby it is made more univerſally uſeful. For this purpoſe it becomes neceſſary to move the marks and eye-pieces of the fixed microſcope, along their reſpective priſms,

to

to the proper poſition for the rod that is to be tried. Nevertheleſs the object lens remains in its original place; and in its ſtead another lens, of the ſame focal diſtance, is fixed on a ſimilar end-piece, that can be firmly clamped to any correſponding place whatever of the cheeks of the long ſlide. Hence will appear the reaſon for breaking the ſcreening tubes of the microſcopes into ſeveral parts, and the uſe of the mahogany priſm, along which the thick part of the tube moves from one end to the other.

The pyrometer, ſince it was firſt made and tried, has undergone ſeveral ſmall alterations, by way of improvements, which it is now unneceſſary to deſcribe particularly. One of theſe was the application of croſs levels to the parts of the tube (SS in the general plan) connected with the object glaſſes. The manner in which they are fixed on will appear from the repreſentations of them in the lowermoſt left-hand angle of the plate. And the ſection at the right-hand angle ſhews the appearance of the double braſs hook, univerſal joint, and milled-headed nut, applied acroſs the middle of the boiler (at TU) whereby the levels are brought to be conſiſtent, when the water is boiling, with the poſition they had been adjuſted to when the temperature was at freezing; that is to ſay, they are kept parallel to themſelves in both ſtates. This was thought neceſſary, becauſe the application of the boiling water ſunk the middle of the ſlide a ſmall matter, and thereby made the levels run outwards.

The micrometer ſo often mentioned, being a very eſſential part of the machine, is repreſented both in elevation and horizontal ſection to the full ſize. Its chief parts conſiſt of a micrometer ſteel ſcrew, which works in the ſquare nut of a braſs ſlide, while the plane part of it enters into a long braſs ſocket,

nicely

nicely ground to receive it, and thereby preventing all ſhake. To the ſquare nut, one end of a watch chain is attached; the other end having paſſed around is fixed to a barrel, which contains a watch ſpring coiled up in the uſual manner. By this contrivance, any loſs of time in the motion of the moveable wire, fixed to the ſquare ſlide, is effectually prevented, whether the ſcrew be turned backwards or forwards. The fixed wire, ſo called becauſe it is only made uſe of occaſionally, appears in the elevation to the left-hand of the former, and is farther removed from the obſerver, being attached to the oval ſlide which bounds the field of the micrometer. This wire is moved by the inſertion of a milled-headed key (although not repreſented in the plate) fitted to ſlip upon the ſquare end of its proper ſcrew, which may be ſeen, in the elevation, projecting above the micrometer head. It has but little motion, being only intended for the meaſurement of ſmall differences of expanſion, or any ſmall ſpace, by leaving it there, while the other wire is repeatedly brought to coincide with, and again depart from it. For particular purpoſes this wire may be uſeful; nevertheleſs, the inſtrument would have performed very well without it.

The conſtruction of the microſcopes will be readily underſtood, by referring to the figures under that head on the right-hand ſide of the plate; where the relative ſituations of the different eye-glaſſes, with regard to the wires or place of the magnified image, as well as to the eye, are truly repreſented in their real dimenſions; but the diſtances from theſe to the object lenſes and marks reſpectively, are contracted or broken off, from want of ſufficient room to delineate them otherwiſe. To increaſe the angle of viſion in microſcopes, it is always neceſſary that they ſhould have at leaſt two eye-glaſſes, and the fixed microſcope in the plate ſhews them in their

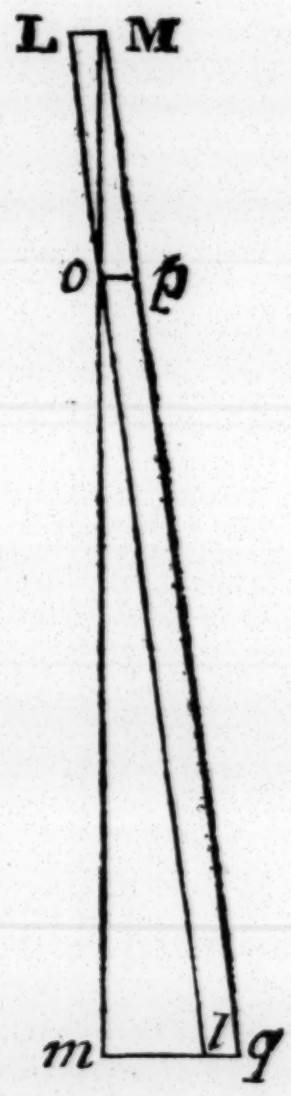
their uſual poſition, the image from the object lens there being formed between the two, that the diſperſion of rays in the firſt may be corrected by that of the ſecond. But although this conſtruction ſerves perfectly well every purpoſe of the fixed microſcope, yet it could not anſwer in the moveable one, to which the micrometer is attached, where equal parts of an image, or their motion, are to be meaſured by the equable motion of the object lens, as ſhewn by the micrometer: for in that caſe, the interpoſition of an eye-glaſs before the image was formed, would not only have diminiſhed its ſize, and thereby rendered the meaſure leſs accurate; but likewiſe, by refracting the oblique pencils more than thoſe nearer the center, it would have deſtroyed the equality of the ſcale, and made equal parts of the object itſelf to have been repreſented unequally in the magnified image, and conſequently erroneouſly meaſured by unequal parts of the micrometer. It was to remedy a defect of this ſort that Mr. RAMSDEN propoſed his new ſyſtem of eye-glaſſes, deſcribed in the Philoſophical Tranſactions, vol. LXXIII. 1783, N° 5. And he has here applied that ſyſtem in the conſtruction of the micrometer microſcope; where it will be perceived, that both glaſſes ſtand between the eye and the image, whereby the greater magnitude of this laſt is obviouſly preſerved, as well as the juſt ſimilarity of all its parts to thoſe of the object itſelf.

With regard to the ſcale of the pyrometer, it is, in the firſt place, to be obſerved, that the head of the micrometer ſcrew, which is nine-tenths of an inch in diameter, is divided into fifty equal parts, each of which being reckoned two, it is therefore numbered to 100. Fifty-five revolutions of the head, being equal to 0.77175 of an inch, as meaſured with great accuracy by Mr. RAMSDEN's ſtraight-line engine, it follows,

follows, that there are 71.27 threads of the ſcrew in an inch; that ſeven revolutions and nearly $\frac{13}{100}$th parts move the wire of the micrometer one-tenth of an inch; and that $\frac{1}{100}$th part of a revolution, or half a diviſion, anſwers to a motion of ſomething more than 0.00014 of an inch.

Having thus obtained the number of revolutions and parts of the micrometer (7.13) correſponding to one-tenth of an inch at the wires, it is ſufficiently obvious, that the number anſwering to one-tenth LM at the mark being likewiſe obtained, and added to the former, their ſum will give the meaſure of one-tenth at the object lens, or the ſpace by which the expanding rod has lengthened, as ſhewn by the motion of the lens from *o* to *p*. This meaſure of one-tenth of an inch at the mark was aſcertained in two different ways, and the reſults exactly agreed with each other. In the firſt place, a very thin ivory ſlide, whereon ſeveral twentieths of an inch were nicely divided by exceeding fine lines, was prepared, and made to move in the mark where the braſs ſlide now exiſts. A candle being then placed behind it at night, while the pyrometer ſtood within doors, and the micrometer wire being repeatedly moved by the ſcrew, its coincidence with the lines was diſtinctly ſeen through the ivory; whereby two of the ſpaces were found to be meaſured by 24.93 revolutions of the head. The ſecond method was, by means of two exceeding fine wires placed parallel to each other on the braſs ſlide, where they now remain, at the diſtance of one-twentieth of an inch on each ſide of the interſection wires, as may be ſeen by obſerving the real mark, or rather its magnified image, as ſhewn in the oval field of the micrometer, in the central figure of conſtruction. The revolutions of the micrometer anſwering to the diſtance between theſe parallel wires was, as before, found to be 24.93; which being

being added to 7.13, we have 32.06 for the number of revolutions meaſuring a motion of one-tenth at the object lens, or the expanſion of one-tenth. In this manner Mr. RAMSDEN obtains the ſcale of his pyrometer in the eaſieſt and moſt ſimple way imaginable, without any neceſſity for knowing the abſolute diſtances of the object lens from the wires of the mark on one hand, and thoſe of the micrometer on the other; diſtances not eaſily aſcertained by actual meaſurement, on account of the poſition of that glaſs in its cell, which cannot conveniently be come at. Thus, in tab. XX. as well as in the annexed figure, LM being the object at the diſtance of the mark, equal to one-tenth of an inch; then *ml* will be its magnified image, in proportion to the former as *mo* is to *o*M. And, if through the point *p*, the place to which the object lens *o* has been carried by the motion of the expanding rod, a line M*q* be drawn parallel to L*l*, we ſhall have *ml* = 24.93 + *lq* = 7.13 = *mq* = 32.06, the number of revolutions of the micrometer meaſuring *op* the expanſion. Having thus obtained the total number of revolutions correſponding to *mq*; and having likewiſe meaſured the total diſtance *m*M = 26.144 inches, a ſpace eaſily aſcertained between the wires of the micrometer and thoſe of the mark, the partial diſtances *mo* and *o*M may then be readily found by computation: for *mq* : *ml* :: *m*M : *mo* = 20.33 inches; and *mq* : *m*M :: *op* : *o*M = 5.814 inches.



In order to finiſh the deſcription of the pyrometer, it is only neceſſary to obſerve farther, that the circular ſcale, ſeen in the elevation of the micrometer, whoſe zero ap-

pears to coincide with the dart on the plane part of the braſs, is that which ſerves by its motion to regiſter the turns of the head. A forked key, fitted to enter the holes near the circumference of the circle, is made uſe of for the adjuſtment of this zero. The circle ſhould never be turned backwards or towards the left, leſt the watch chain ſhould thereby be thrown off the barrel, but always forwards or towards the right, even if it ſhould be neceſſary to move it almoſt an entire revolution. The zero of the head is that which ſhould be firſt brought to correſpond with its proper dart. They may be ſeen to coincide in the horizontal ſection of the micrometer; and the departure of zero from this dart, indicates, by the number of diviſions that are intercepted, the value of any fractional part of a revolution.

Account of the experiments with the Pyrometer.

Although the inſtrument which I have here endeavoured to deſcribe was begun early in the winter of 1784, yet it was not finiſhed till the beginning of laſt April; at which time it was brought to Argyll-Street, and being placed truly level on the ſtone pavement of the yard, was covered with an oil-cloth canopy, that the experiments might not be interrupted by rainy weather.

To fill the three troughs completely it required from twenty-five to thirty pounds of pounded ice, which was always put in with great care, ſo as to apply as compactly as poſſible to the ſtandard priſms and rod reſpectively, with but little common water *

* When common water was uſed, although not in any very conſiderable proportion, the thermometer kept always half, and ſometimes three quarters of a degree above 32°.

at firſt added; it having been found in theſe experiments, that ice water only, ſuch as drains from the ice itſelf, is that which ſhould properly be made uſe of to mix with the pounded ice, in order to bring the whole maſs to the true freezing temperature. Being at the commencement uncertain what time might be neceſſary for the rods, eſpecially when of ſo large a ſize as the ſtandard priſms, to acquire the juſt temperature of freezing, at firſt the ice was put into the troughs over night, to prepare for the continuation of the experiment next morning. But after many repeated trials, this precaution was found to be needleſs; a quarter of an hour being more than ſufficient to give to all the freezing temperature, as well as to render the lens on the expanding rod ſtationary, after the water ſupplying the place of the ice had been brought fairly to boil.

The inſtrument, in its firſt ſtate, having in ſome caſes made the expanſion appear to be progreſſive, and not equable; therefore its rate was attempted to be aſcertained by noting the progreſſion anſwering to 60°, 120°, and 180° above freezing. But when the inſtrument was rendered perfect, and that no ſenſible difference was found between the expanſion at the lower and that at the upper part of the ſcale, a fair mean being taken between its aſcending and deſcending rates, and allowing for the difficulty of keeping the water, for any length of time, preciſely to the ſame intermediate heat; then this tedious mode of conducting the experiments was given up, and the expanſion for 180° was at once determined by bringing the water to boil around that rod, which but a little before had been lying in melting ice, and which the ſtandard priſms ſtill continued to do throughout each experiment, care being taken to have a ſupply of pounded ice always ready to keep theſe two troughs quite full.

Two obſervers are neceſſary for the effectual application of the pyrometer. He who obſerves with the fixed microſcope, takes care that its object lens is kept in its true place, that is to ſay, that the wire in the eye-piece accurately biſects the interſection wires of the mark. This he is enabled to do by means of the apparatus attached to the fixed end of the boiler, as will be beſt conceived by obſerving the plan (at WX) along with the elevation of that end placed near it. The apparatus conſiſts of two milled-headed ſcrews, working in braſs plates faſtened to the end of the frame, and acting againſt a ſmall cock which projects from the lower part of the boiler, whereby this laſt receives ſuch longitudinal motion to and fro on its rollers, as is ſufficient for the adjuſtment of the lens. He who obſerves with the micrometer microſcope, having brought the zero of the micrometer head to its dart, as ſhewn in the horizontal ſection, and alſo the revolution zero to its dart, as repreſented in the elevation, takes care, when the rod has acquired the freezing temperature, that the micrometer wire biſects the interſection wires of its proper mark. This he effects by working with the milled-headed ſcrew, repreſented in the plan and elevation of that mark, whereby the mark itſelf is moved until the biſection is accurate; and during the whole of this time, the firſt obſerver muſt be extremely attentive to keep his lens adjuſted.

One aſſiſtant at leaſt is neceſſary, who takes his ſtation on the oppoſite ſide of the pyrometer, to obſerve the levels, and keep them adjuſted, by means of the double hook applied near the middle of the boiler, and repreſented in the ſection on the line TU, at the lowermoſt right-hand angle of the plate.

The pyrometer having been adjuſted in the manner here deſcribed, by giving ſufficient time for the ſtandard priſms and

rod to contract to the true freezing temperature, as was eaſily known by the wires becoming perfectly fixed and ſtationary with regard to the marks; the ice was then removed from the copper trough; and the ſame being filled with water nearly on the boil, the ebullition was completed, and kept up, by means of the lamps now lighted for the purpoſe, and ſlipped in underneath.

The expanſion, anſwering to the 180° between freezing and boiling, was now meaſured by working with the micrometer ſcrew until the biſection * of its wire with thoſe of the mark was again complete; the obſerver at the fixed microſcope taking alſo eſpecial care all the while to keep his biſection perfectly accurate. The number of revolutions, regiſtered by the number of entire diviſions that the zero of the circular ſcale had departed from its dart or index, and alſo the value of any fractional revolution, regiſtered by the diviſions on the head intercepted between zero and its proper dart, were then noted, as expreſſed in the firſt column of the ſubjoined table of experiments; which requires no other explanation than what is therein inſerted, and which has been extended purpoſely to ſhew at one view, from inſpection only, how much the length of our baſe would have been affected, if meaſured by theſe metals reſpectively, in temperatures between 32° and 62°.

All the experiments were repeated at leaſt twice, and ſome of them three times, except the ſtandard ſcale and glaſs pen-

* This biſection of the wires may always be made to a great degree of preciſion, by one with a tolerably good eye, and accuſtomed to obſervations of this ſort. I have myſelf repeatedly adjuſted the wires eight or ten times running, allowing another perſon to read off and unadjuſt each time, without the mean difference exceeding one-fourth of a diviſion of the head, which is only $\frac{1}{64000}$th part of an inch.

dulum

dulum rod, whoſe expanſions were only tried once. The difference of a few diviſions between the mean and extremes on the heat of 180° being, in things of this ſort, of no importance, it was judged wholly unneceſſary to aim at a greater degree of preciſion in repeating them oftener. By referring to the table, particularly that column containing the expanſions on one foot by 180°, it will be perceived, that they are uniformly a ſmall matter leſs than what has been aſſigned to the ſame metals reſpectively, in the experiments formerly alluded to.

Ultimate determination of the length of the Baſe on Hounſlow-Heath.

In the former part of this paper, we have had occaſion to ſpeak of the ſeven firſt columns of the general table of the baſe; and the titles at the tops of the others reſpectively ſerve ſufficiently to explain thoſe towards the right-hand; the expanſion of glaſs above, and its contraction below 62°, contained in the eleventh and twelfth columns, being deduced from the recent experiments with the pyrometer.

	Feet.
The hypothenuſal length of the baſe, as meaſured by 1369.925521 glaſs rods of twenty feet each + 4.31 feet, being the diſtance between the laſt rod and the center of the north-weſt pipe, has been ſhewn to be - - -	27402.8204
The reduction contained in the ſeventh column of the general table to be deducted is : .	0.0714
Hence the apparent length of the baſe, reduced to the level of the ſouth-eaſt extremity, becomes	27402.7490

The

	Feet.
The apparent length is to be augmented by the excess of the expansion above the contraction of the glass rods, contained in the thirteenth column of the general table = 4.1867 inches, reduced to the heat of 62°, as has been usually done in former operations of this nature . . .	0.3489
The apparent length is farther to be augmented by the equation for 6° difference of temperature of the standard brass scale between 62° and 68°, this last being the heat in which the lengths of the glass rods were laid off = 20.3352 inches, as deduced from the experiments with the pyrometer	1.6946
Hence we have the correct length of the base in the temperature of 62° reduced to the level of the lowermost extremity near Hampton Poor-house,	27404.7925
This last length requires yet a small reduction for the height of this lowermost end above the mean level of the sea, supposed to be fifty-four feet, or nine fathoms, . . .	0.0706
Hence the true or ultimate length of the base, reduced to the level of the sea, and making a portion of the mean circumference of the earth, becomes	27404.7219

As some small degree of uncertainty remains with regard to this last reduction, it may not be improper to say yet a few words on the principles that have been adhered to in making the computation. It will be remembered, that the measurement was made $3\frac{1}{2}$ feet above the surface of the heath, that being the height of the stands whereon the rods were placed;

and

and that the teleſcopic ſpirit level gave a deſcent of 36.1 feet from the lowermoſt pipe to the ſurface of ſummer water in the Thames at Hampton. The accurate ſection of the river lately publiſhed, gives a fall of 13.33 feet from Hampton to the level of low water ſpring tides at Iſleworth. Now theſe three being added together, we have nearly fifty-three feet for the height of the baſe above Iſleworth. Having had no immediate means of determining what real difference there may be between Iſleworth and low water ſpring tides at the mouth of the Thames (for inſtance at the Hope or the Nore), I have ſuppoſed that fall to be about ſeven feet, ſo as to make the total deſcent ſixty feet. Now, ſuppoſing the ſpring tides at the Nore to riſe eighteen feet, if, according to M. DE LA LANDE's method, we deduct one-third of eighteen, *viz.* ſix feet from ſixty, we ſhall have fifty-four feet, or nine fathoms, that the mean ſurface of the ſea is below the meaſured baſe. Whether this concluſion be perfectly accurate or not is of no moment, ſince a whole fathom of difference (and I apprehend we are not farther from the truth) does not vary the reduction quite one-tenth of an inch. The reduced baſe has therefore been found by the following analogy: as the mean ſemi-diameter of the earth (ſuppoſed here to be 3492915 fathoms) augmented by nine fathoms, is to the mean ſemi-diameter, ſo is the meaſured baſe 27404.7925 to the reduced baſe 27404.7219 at the level of the ſea. It will doubtleſs be allowed, that infinite pains have been taken in the field and otherwiſe, throughout the whole of this operation, to obtain a juſt concluſion; but as the moſt accurate meaſurement imaginable is ſtill more liable to err in exceſs than in defect, we will throw away ſome uſeleſs decimals, and eſtabliſh the ultimate length of the baſe at 27404 feet and ſeven-tenths.

General

General Table of the Base, shewing the relative heights of the Stations above the south-east extremity near Hampton Poor-house, the Reduction of the Hypothenuses, and the Correction for the Temperature of the Glass Rods; whence the true length is obtained in the heat of 62°.

1	2	3	4	5	6	7	8	9	10	11	12	13	14
Sections.	N° of hypothenuses, 600 feet each.	Relative heights.				Reduction of the hypothenuses.	Number of rods, twenty feet each.	Temperature.		Correction for temperature.			Correct length of the base.
		Ascent.	Descent.	Total ascent. Hypoth.	Total ascent. Sections.			Observed mean of 60 therm.	Excess or defect from 62°.	Expansion above 62° + appt. leng.	Contract. below 62° — appt. leng.	Difference.	
	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.		°	°	Inches.	Inches.	Inches.	Feet.
First or south-east section, between Hampton Poor-house and Hanworth Summer-house.	1	0.07	— —	0.07		0.000012	29+ .925521	71 4	+ 9.4	+0.2909			
	2	1.855	— —	1.925		0.002875	30	80 7	+18.7	+0.5801			
	3	— —	1.855	0.07		0.002875	30	80.6	+18.6	+0.5770			
	4	2.745	— —	2.815		0.006288	30	74.0	+12.0	+0.3722			
	5	2.92	— —	5.735		0 007114	30	62 6	+ 0.6	+0.0186			
	6	0.76	— —	6.495		0.000489	30	58.7	— 3.3		—0.1024		
	7	1.57	— —	8.065		*0.000825	30	60.5	— 1.5		—0.0465		
	8	2.91	— —	10.975		0.007065	30	63.3	+ 1.3	+0.0403			
	9	— —	0.68	10.295		0.000393	30	64.1	+ 2.1	+0.0651			
	10	0.65	— —	10.945		0.000360	30	66.6	+ 4.6	+0.1427			
	11	— —	0.04	10.905		0.000009	30	70.4	+ 8.4	+0.2606			
	12	— —	1.18	9.725		0.001168	30	69 9	+ 7.9	+0.2451			
	13	0.83	— —	10.555	10.555	0.000581	30	62.7	+ 0.7	+0.0217			
Second or middle section, between Hanworth Summer-house and the north side of the Staines Road.	14	— —	0.94	9.615		0.000745	30	60.7	— 1.3		—0.0403		
	15	0.42	— —	10.035		0.000191	30	63.2	+ 1.2	+0.0372			
	16	— —	1.63	8.405		0.002222	30	69.0	+ 7.0	+0.2171			
	17	0.28	— —	8.685		0.000073	30	71.1	+ 9.1	+0.2823			
	18	4 16	— —	12.845		0.014439	30	68.6	+ 6.6	+0.2047			
	19	— —	0.44	12.405		0.000169	30	63.5	+ 1.5	+0.0465			
	20	0.19	— —	12.595		0.000036	30	59.2	— 2.8		—0.0869		
	21	1.87	— —	14 465		0.002922	30	56.2	— 5.8		—0.1799		
	22	0.73	— —	15.195		0.000452	30	57.0	— 5 0		—0.1551		
	23	0.39	— —	15 585		0.000135	30	64.1	+ 2.1	+0.0651			
	24	0.95	— —	16.535		0.000760	30	64.3	+ 2.3	+0.0713			
	25	0.49	— —	17.025		0·000208	30	62.5	+ 0.5	+0.0155			
	26	2.11	— —	19.135	8.58	*0.000063	30	71.1	+ 9.1	+0.2823			
Third or north-west section, between the north side of the Staines Road, and King's Arbour near the Colnbrook Road.	27	— —	0.71	18.425		*0.000214	30	70.5	+ 8.5	+0.2637			
	28	0.245	— —	18.67		0.000057	30	63.3	+ 1.3	+0.0403			
	29	1.21	— —	19.88		0.001228	30	59.2	— 2.8		—0.0869		
	30	— —	0.165	19.715		0.000028	30	66.2	+ 4.2	+0.1303			
	31	0.14	— —	19.855		0.000024	30	73.8	+11.8	+0.3660			
	32	— —	0.12	19.735		0.000019	30	77.6	+15.6	+0.4839			
	33	— —	0.14	19.595		0.000024	30	73.7	+11.7	+0.3629			
	34	1.21	— —	20.805		0.001228	30	68.8	+ 6.8	+0.2109			
	35	1.405	— —	22.21		0.001653	30	65.8	+ 3.8	+0.1179			
	36	2.34	— —	24.55		0.004571	30	65.5	+ 3.5	+0.1086			
	37	— —	1.085	23.465		0.000985	30	61.5	— 0 5		—0.0155		
	38	0.47	— —	23.935		0.000192	30	59.4	— 2.6		—0.0807		
	39	0.525	— —	24.46		0.000238	30	55.6	— 6.4		—0.1985		
	40	1.265	— —	25.725		0.001341	30	55.1	— 6.9		—0.2140		
	41	1.18	— —	26.905		0.001168	30	56.1	— 5.9		—0.1830		
	42	— —	0.19	26.715		0.000036	30	58.4	— 3.6		—0.1117		
	43	1.565	— —	28.28		0.002049	30	58.2	— 3.8		—0.1179		
	44	1.485	— —	29.765		0.001845	30	57.3	— 4.7		—0.1458		
	45	0.24	— —	30.005		0.000055	30	60.8	— 1.2		—0.0372		
	46	1.26	— —	31.265	12.130	0.001973	20	65.3	+ 3.3	+0.0682			
	400 ft.												
		40.44	9.175	31.265	31.265	0.071401	1369+ .925521			+5.9890	—1.8023	+4.1867	

Hypothenusal length of the base containing 1369.925521 glass rods of twenty feet each + 4.31 feet,	=	27402.8204
Reduction contained in the seventh column to be subtracted,	—	0.0714
Total apparent length of the base reduced to the level of the south-east extremity,		27402.7490
Add to the apparent length the difference between the expansion of glass above, and the contraction of it below 62°, contained in the thirteenth column = 4.1867 inches =	+	0.3489
Add further to the apparent length the equation for 6° difference of temperature of the standard brass scale between 62° and 68°, the heat in which the glass rods were laid off = 20.3352 inches, =	+	1.6946
Correct length of the base in the temperature of 62°, reduced to the level of the lowermost extremity,	=	27404.7925
Reduction for the height of the lower end of the base above the mean level of the sea, supposed to be 54 feet or 9 fathoms =	—	0.0706
True length of the base reduced to the mean level of the sea,	=	27404.7219

Standard ſcal
Engliſh braſs, of a ro
Engliſh braſs, i of a tr
Steel r
Caſt pri
Glaſs
Solid gl

1
2
3
4
5

April 1785.

Description		eet.	1000 ft.	Baſes of 27400 feet of theſe metals would expand. By 1°.	By 10°.	By 20°.	By 30°.
			In.	In.	In.	In.	In.
Standard braſs ſcale.	42 in 1(re or	22	0.1237	3.38938	33.8938	67.7876	101.6814
Engliſh plate braſs, in form of a rod.	n(D w	72	0.1262	3.45788	34.5788	69.1576	103.7364
Engliſh plate braſs, in form of a trough.	in ſt	578	0.1263	3.46062	34.6062	69.2124	103.8168
Steel rod.	o: vi	578	0.0763	2.09062	20.9062	41.8124	62.7186
Caſt iron priſm.	a: r(	440	0.0740	2.02760	20.2760	40.5520	60.8280
Glaſs tube.	w p	102	0.0517	1.41658	14.1658	28.3316	42.4974
Solid glaſs rod.	n 2 c n o	234	0.0539	1.47686	14.7686	29.5372	44.3058

Table of the Expanſions of Metals, deduced from experiments made

Deſcription of the Metal Rods put to experiment.	Revolutions and parts of the micrometer for the expanſion on five feet.		Actual expanſion in parts of an inch by 180°, the revolutions being divided by 32.06.			
	By 180°	By 1°.	On 5 feet.	On 1 foot.	On 100 ft.	On 1
	Rev.Pts.	Parts.	In.	In.	In.	In.
Standard braſs ſcale. — Suppoſed to be Hamburgh plate braſs; length 42.187 inches, or 3.568 feet; breadth 0.55 inch; thickneſs 0.25 inch; and weight 1 lb. 10½ oz. Its expanſion was meaſured by 25.47 revolutions of the micrometer; wherefore that on five feet would have been meaſured by -	35.69	19.$\frac{83}{100}$	0.111323	0.0222646	2.22646	0.000
Engliſh plate braſs, in form of a rod. — Length five feet; breadth 0.9 inch; thickneſs 0.15 inch; and weight 2 lbs. 5½ oz. Difficult from its thinneſs to be kept free from warping, - - -	36.41	20.$\frac{23}{100}$	0.113568	0.0227136	2.27136	0.000
Engliſh plate braſs, in form of a trough. — Length five feet; breadth 1.4 inch; depth 1 inch; weight 8 lbs. 3 oz. Perfectly ſtrong and ſtraight, - - -	36.45	20.$\frac{25}{100}$	0.113693	0.0227386	2.27386	0.000
Steel rod. — Length five feet; breadth 0.5 inch; thickneſs 0.3 inch; weight 2 lbs. 7½ oz. Made from the very ſame bar with the chain, - -	22.02	12.$\frac{23}{100}$	0.068684	0.0137368	1.37368	0.000
Caſt iron priſm. — Length five feet; each of its ſides 1¼ inch; and weight 11 lbs. 9 oz. Cut from the ſame rod with the ſtandard priſms of the pyrometer,	21.34	11.$\frac{86}{100}$	0.066563	0.0133126	1.33126	0.000
Glaſs tube. — Length five feet; $\frac{93}{150}$ths inch diameter; weight 1 lb. 13½ oz. Drawn from the ſame pot of metal with the meaſuring rods, -	14.93	8.$\frac{29}{100}$	0.046569	0.0093138	0.93138	0.000
Solid glaſs rod. — Length 40.44 inches, or 3.37 feet; mean diameter ſix-tenths of an inch; and weight 1 lb. 2 oz. It had been applied for ſeveral years to a clock. Its expanſion was meaſured by 10.46 revolutions of the micrometer; wherefore that on five feet would have been meaſured by -	15.54	8.$\frac{63}{100}$	0.048472	0.0096944	0.96944	0.000

ents made with the Microſcopic Pyrometer in April 1785.

of an utions	By 1° of FAHRENHEIT the expanſion is						Baſes of 27400 feet of theſe metals would expand.			
100 ft.	On 1 foot.	10 feet.	100 feet.	400 feet.	600 feet.	1000 ft.	By 1°.	By 10°.	By 20°.	By 30°.
	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.
2646	0.0001237	0.001237	0.01237	0.04948	0.07422	0.1237	3.38938	33.8938	67.7876	101.6814
7136	0.0001262	0.001262	0.01262	0.05048	0.07572	0.1262	3.45788	34.5788	69.1576	103.7364
7386	0.0001263	0.001263	0.01263	0.05052	0.07578	0.1263	3.46062	34.6062	69.2124	103.8168
7368	0.0000763	0.000763	0.00763	0.03052	0.04578	0.0763	2.09062	20.9062	41.8124	62.7186
3126	0.0000740	0.000740	0.00740	0.02960	0.04440	0.0740	2.02760	20.2760	40.5520	60.8280
93138	0.0000517	0.000517	0.00517	0.02068	0.03102	0.0517	1.41658	14.1658	28.3316	42.4974
6944	0.0000539	0.000539	0.00539	0.02156	0.03234	0.0539	1.47686	14.7686	29.5372	44.3058

Table of the Expansions of Metals, deduced from experiments made

Description of the Metal Rods put to experiment.	Revolutions and parts of the micrometer for the expansion on five feet.		Actual expansion in parts of an inch by 180°, the revolutions being divided by 32.06.			
	By 180°	By 1°.	On 5 feet.	On 1 foot.	On 100 ft.	On 1
	Rev.Pts.	Parts.	In.	In.	In.	In.
Standard brass scale. — Supposed to be Hamburgh plate brass; length 42.187 inches, or 3.568 feet; breadth 0.55 inch; thickness 0.25 inch; and weight 1 lb. 10½ oz. Its expansion was measured by 25.47 revolutions of the micrometer; wherefore that on five feet would have been measured by -	35.69	19.$\frac{83}{100}$	0.111323	0.0222646	2.22646	0.000
English plate brass, in form of a rod. — Length five feet; breadth 0.9 inch; thickness 0.15 inch; and weight 2 lbs. 5½ oz. Difficult from its thinness to be kept free from warping, - - -	36.41	20.$\frac{23}{100}$	0.113568	0.0227136	2.27136	0.000
English plate brass, in form of a trough. — Length five feet; breadth 1.4 inch; depth 1 inch; weight 8 lbs. 3 oz. Perfectly strong and straight, - - -	36.45	20.$\frac{25}{100}$	0.113693	0.0227386	2.27386	0.000
Steel rod. — Length five feet; breadth 0.5 inch; thickness 0.3 inch; weight 2 lbs. 7½ oz. Made from the very same bar with the chain, - -	22.02	12.$\frac{23}{100}$	0.068684	0.0137368	1.37368	0.000
Cast iron prism. — Length five feet; each of its sides 1¼ inch; and weight 11 lbs. 9 oz. Cut from the same rod with the standard prisms of the pyrometer,	21.34	11.$\frac{36}{100}$	0.066563	0.0133126	1.33126	0.000
Glass tube. — Length five feet; $\frac{93}{1000}$ths inch diameter; weight 1 lb. 13½ oz. Drawn from the same pot of metal with the measuring rods, -	14.93	8.$\frac{29}{100}$	0.046569	0.0093138	0.93138	0.000
Solid glass rod. — Length 40.44 inches, or 3.37 feet; mean diameter six-tenths of an inch; and weight 1 lb. 2 oz. It had been applied for several years to a clock. Its expansion was measured by 10.46 revolutions of the micrometer; wherefore that on five feet would have been measured by -	15.54	8.$\frac{63}{100}$	0.048472	0.0096944	0.96944	0.000

nents made with the Microscopic Pyrometer in April 1785.

s of an lutions	By 1° of FAHRENHEIT the expansion is						Bases of 27400 feet of these metals would expand.			
100 ft.	On 1 foot.	10 feet.	100 feet.	400 feet.	600 feet.	1000 ft.	By 1°.	By 10°.	By 20°.	By 30°.
	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.
22646	0.0001237	0.001237	0.01237	0.04948	0.07422	0.1237	3.38938	33.8938	67.7876	101.6814
27136	0.0001262	0.001262	0.01262	0.05048	0.07572	0.1262	3.45788	34.5788	69.1576	103.7364
27386	0.0001263	0.001263	0.01263	0.05052	0.07578	0.1263	3.46062	34.6062	69.2124	103.8168
37368	0.0000763	0.000763	0.00763	0.03052	0.04578	0.0763	2.09062	20.9062	41.8124	62.7186
33126	0.0000740	0.000740	0.00740	0.02960	0.04440	0.0740	2.02760	20.2760	40.5520	60.8280
93138	0.0000517	0.000517	0.00517	0.02068	0.03102	0.0517	1.41658	14.1658	28.3316	42.4974
96944	0.0000539	0.000539	0.00539	0.02156	0.03234	0.0539	1.47686	14.7686	29.5372	44.3058

Granford Bridge Inn

Granford Field

Harlington

Harlington Fidd

London

Hatton Fidd

C R A N F O R D

H E

H A R L I N G T O N P A R I S H

Coach & Horses

Colnbrook

Sipson Green

from Work House

Hatton

H A R M O N D S W O R T H P A R I S H

Schapsbury Hill

Fern Hill

Road

King's Arbour

Magpye

45 44 43 42 41 40 39 38 37 36 35 34 33 32

O

H

Heath

Row

Heath Row Field

Hatton

OLD RIVER

STANWELL P.

B E D

Bedf

Scale of Two Miles.

1 2 3 4 5

PLAN ſhewing the ſituation of the BASE meaſured o

HESTON PARISH

CRANFORD RIVER

LONDON

ISLEWORT

Powder Mills

Brazil Mills

Feltham Bridg

PARISH

tton

Mailman's Bridge

S TAINES to

N S L O

U

33 32 31 30 29 28 27 26 25 24 23 22 21 20 19 18

from

WAY

RIVER

a BRANCH of the COLN

PARISH

WOLSEY

ROMAN

a BRANCH of the COLN

FELTHAM

H

Hatton Bridges

IVER

BEDFOND

Bedfond

Feltham

Feltham Field

1 Mile

1000 500 0 1000 2000 3000

the BASE measured on HOUNSLOW HEATH in Summer 1784.

ISLEWORTH P.

TWICKENHAM P.

PARISH

Hospital Bridge

New Powder Mills

Feltham Bridge

Brazil Mills

Hanworth Farm

LOWER HANWORTH PARISH

22 21 20 19 18 17 16 15 14 13 12 11 10 9 8 7 6 5 4

Summer House

BRANCH of the COLN

HANWORTH PARK

Little Lane

Long Lane

Feltham

Church

Hanworth House

Hanworth

Scale of Feet.

1000 2000 3000 4000 5000 6000 7000 8000 9000 10,000 Feet. 100

Oil Mill House

Bridge

Lower Lodge

Upper Lodge

Poor Houn

Love Lane

Chalk Farm

Little Lane

Cross Lane

Broad Lane

Holly Bush Lane

Clea Lane

Old Field Lane

Hampton

KENTON PARK

BUSHEY PARK

RIVER THAMES

MOULSEY HURST

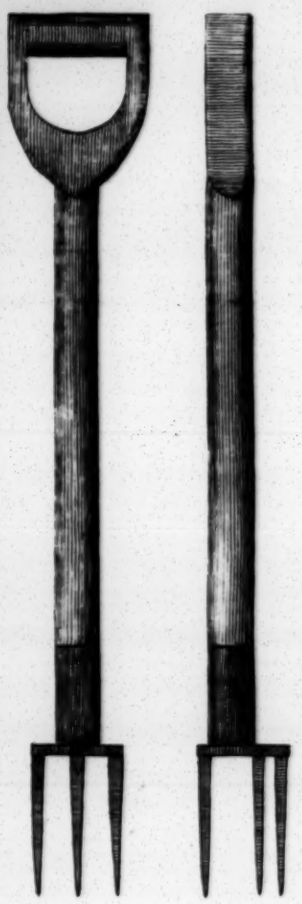
Scale of Fathoms & Toises.

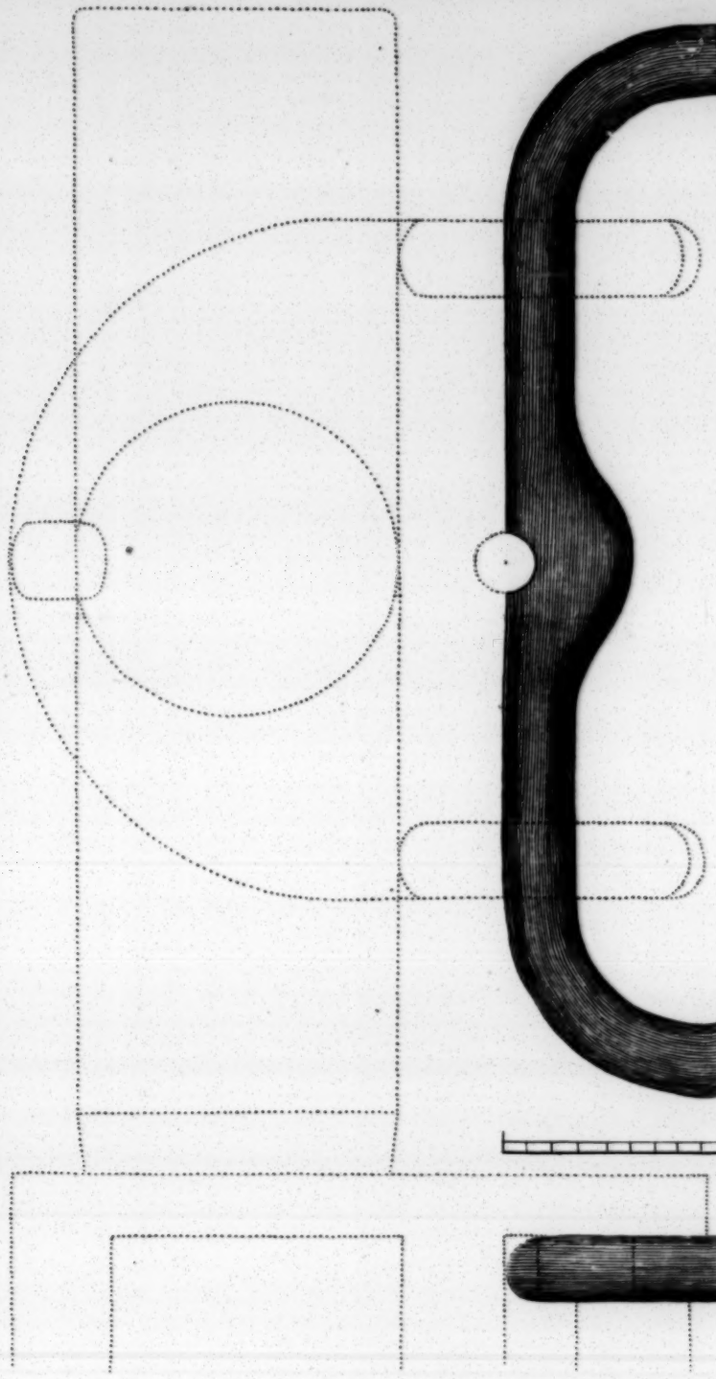
1000 Fathoms.

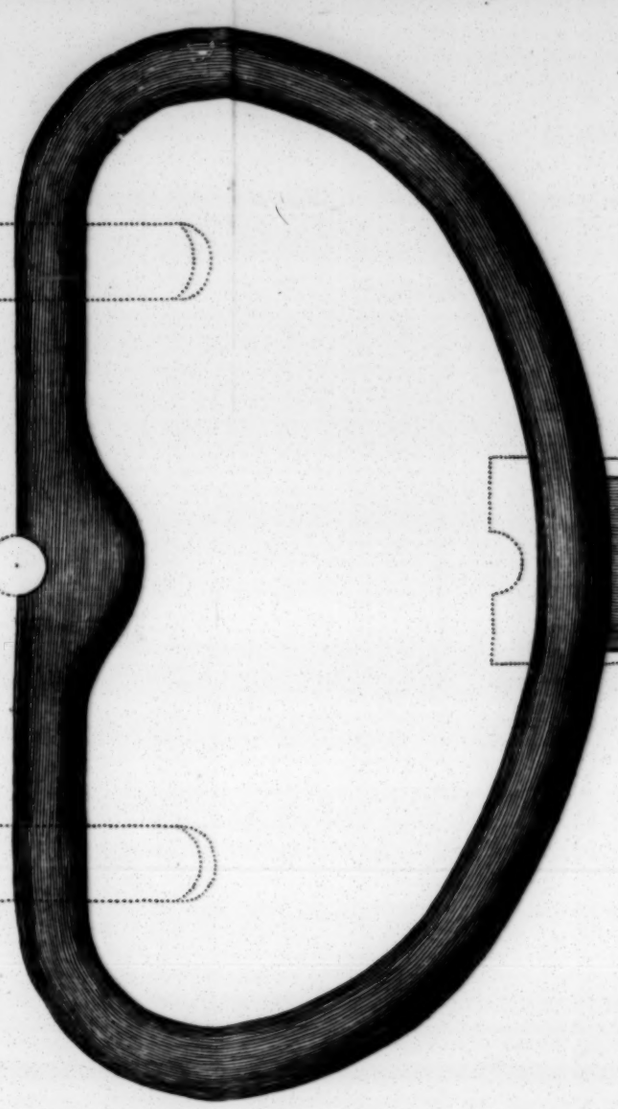
1000 Toises.

For th

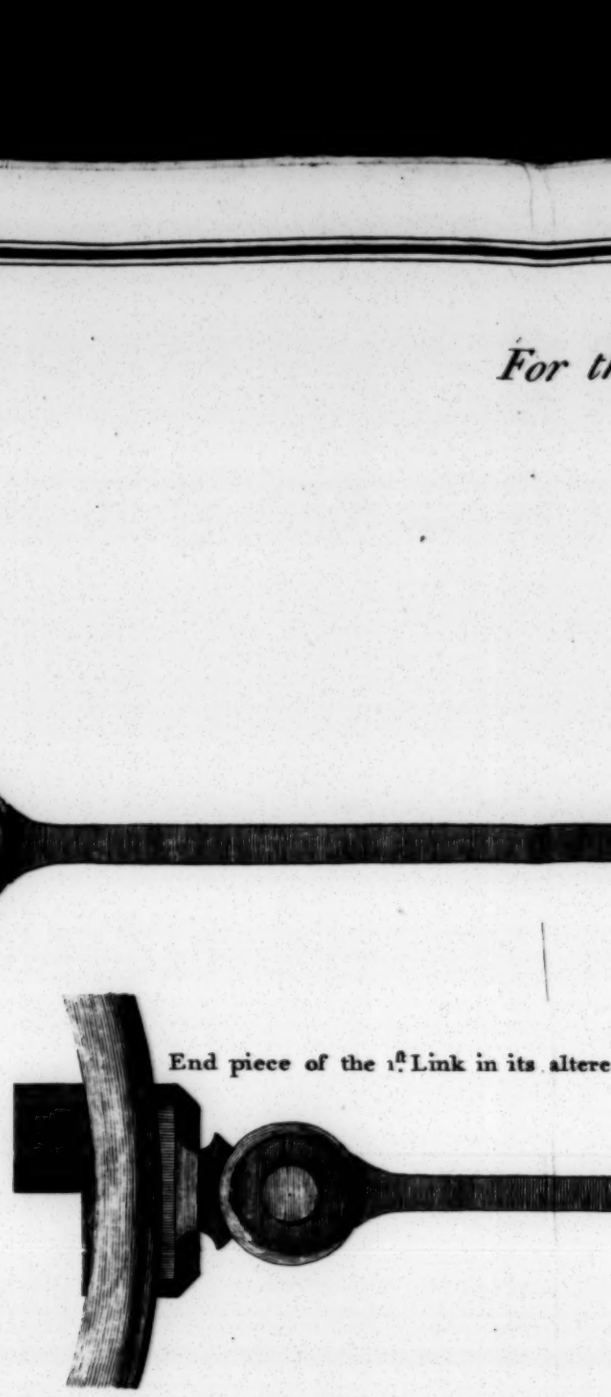
Holdfaſt for the 1.st End of the Chain.

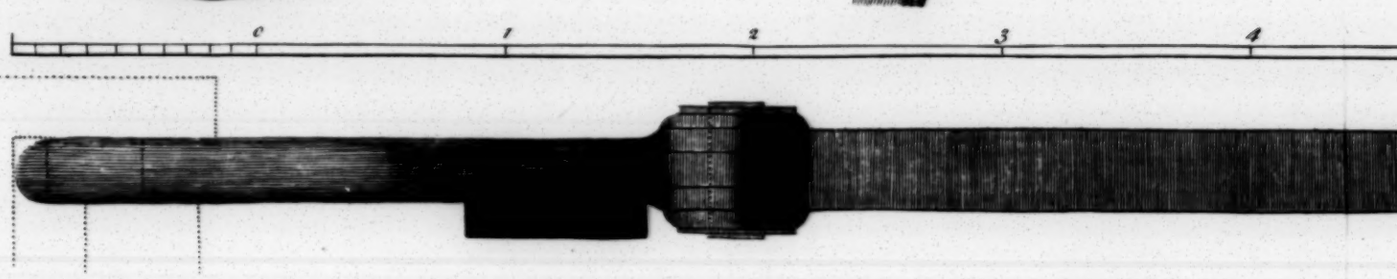






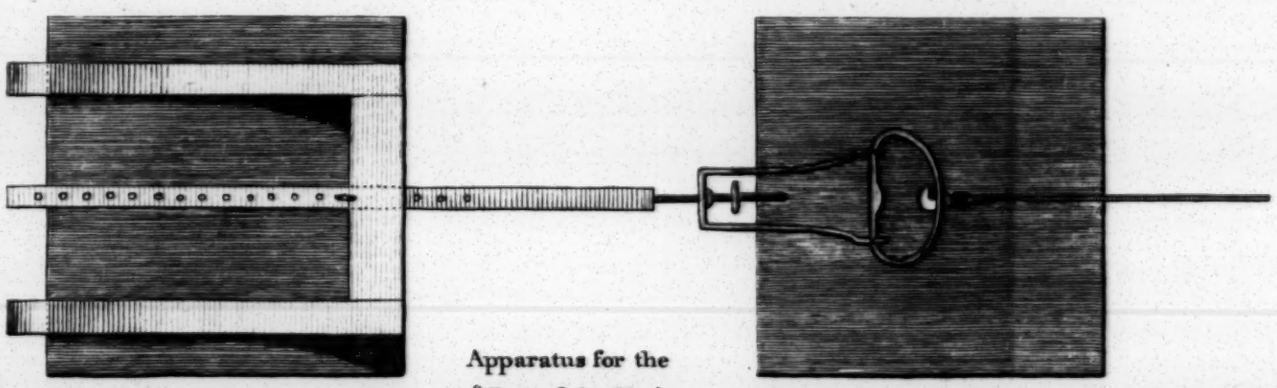
End piece of the 1.st Link in its altere



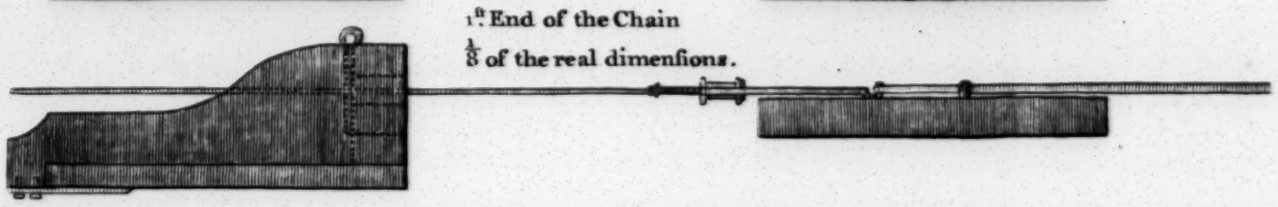


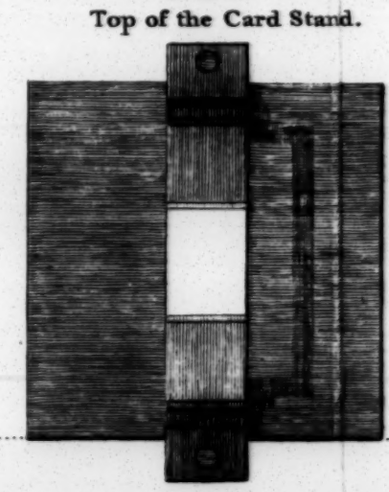
Difpoſition of Seventeen Stands for the double Meaſuremen

Top of the Card Stand.

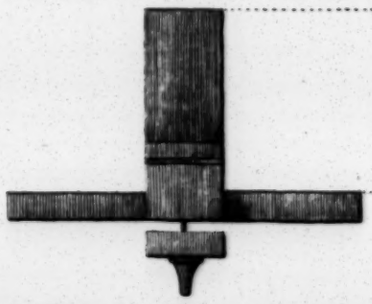


Apparatus for the
1.st End of the Chain
$\frac{1}{8}$ of the real dimenſions.





Side Elevation of the Card Stand.

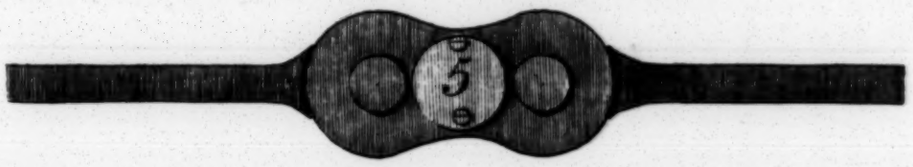


For the DESCRIPTION and APPLICATION of the STEEL CHAIN.

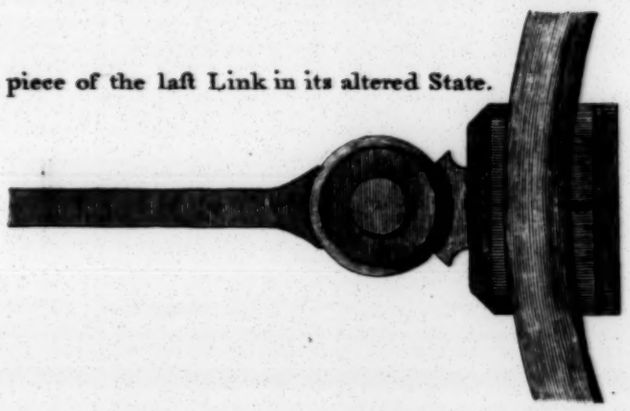
Plan of the 1st Link of the Chain in its original State.

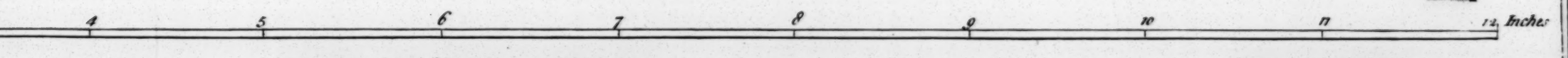
the 1st Link in its altered State.

Plan of the Croſs joint at every 10th Link.



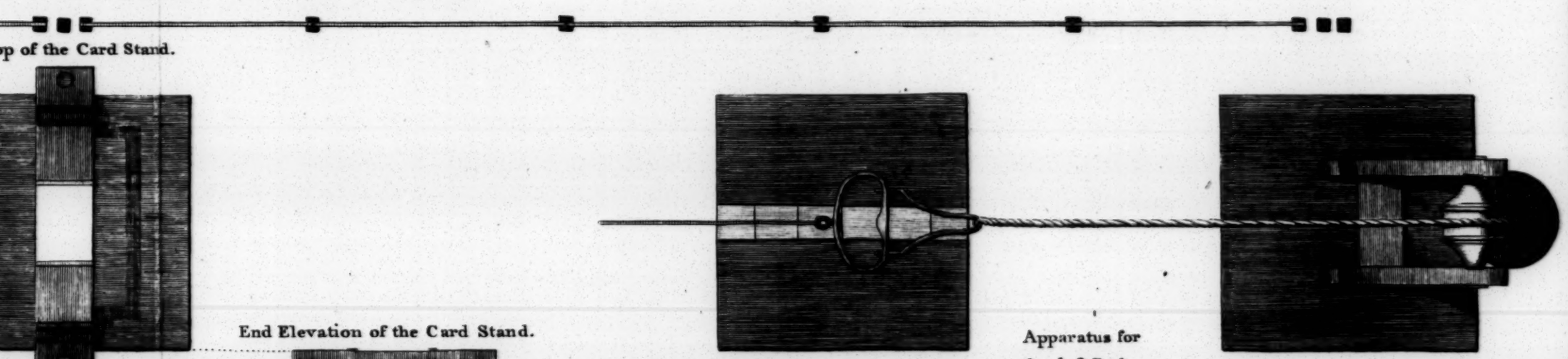
End piece of the laſt Link in its altered State.





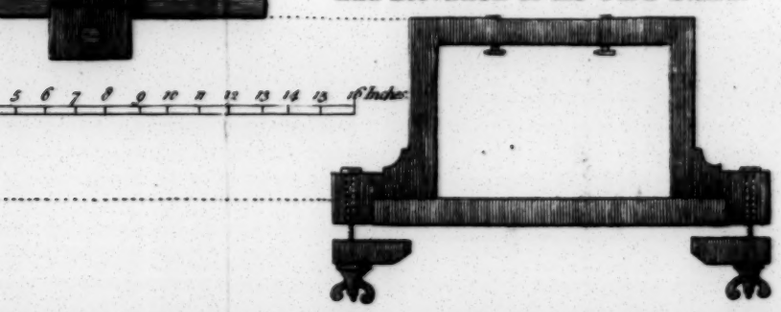
Elevation of the 1st Link in its original State.

he double Meaſurement with the Chain and Glaſs Rods.

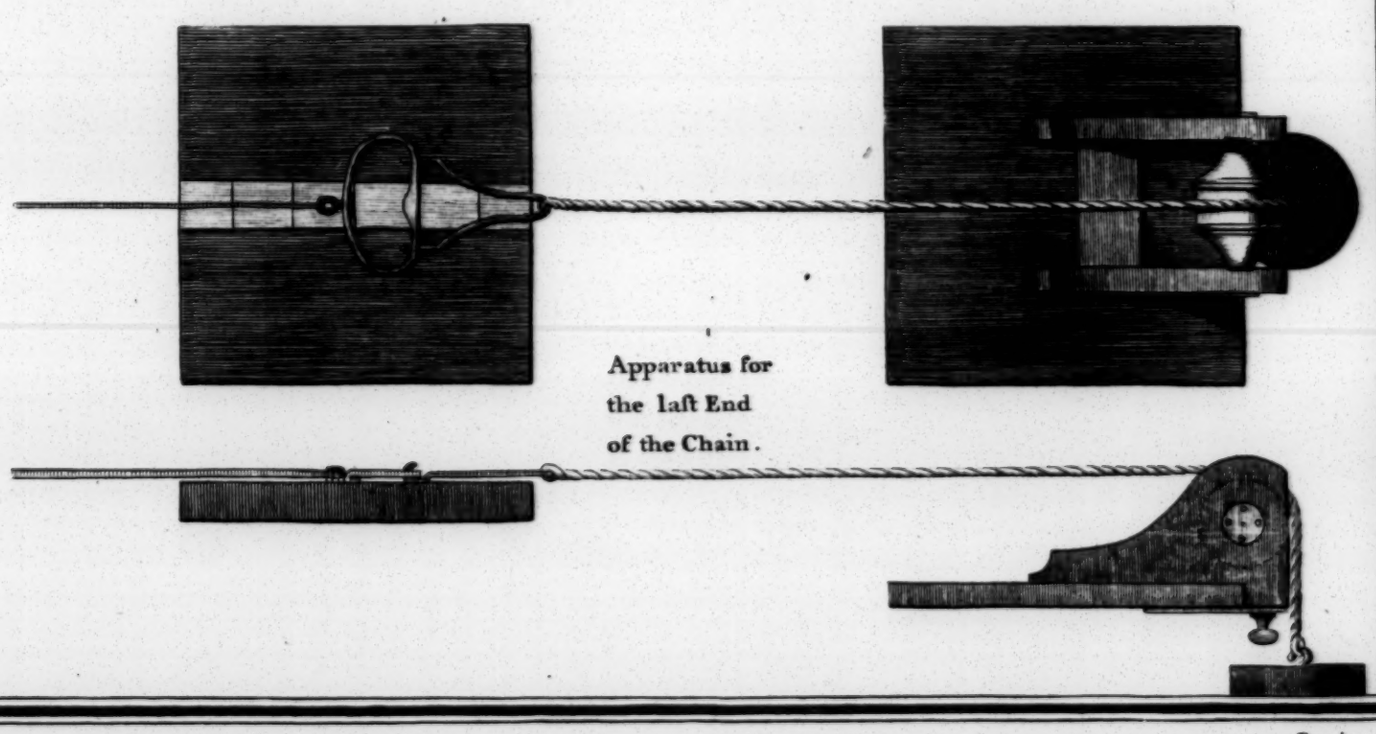


p of the Card Stand.

End Elevation of the Card Stand.

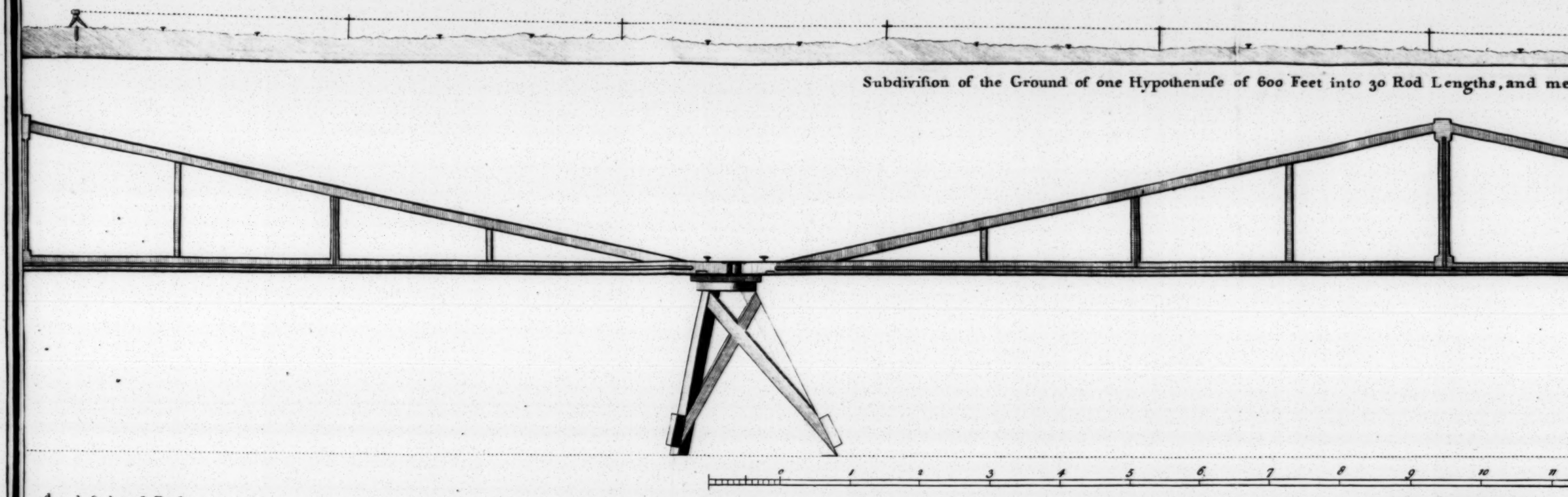


Apparatus for the laſt End of the Chain.



Basire ſc.

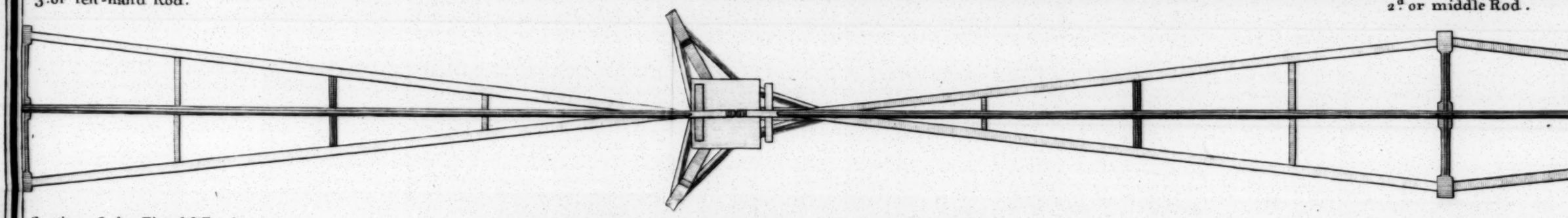
For the DESCRIPTION *and* APPLICATION *o*



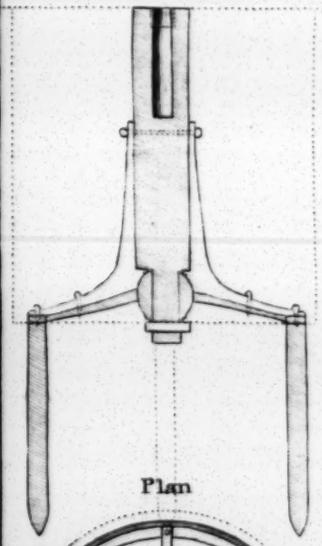
Subdivision of the Ground of one Hypothenuse of 600 Feet into 30 Rod Lengths, and me

3d. or left-hand Rod.

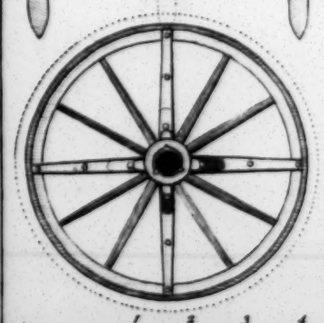
2d. or middle Rod.



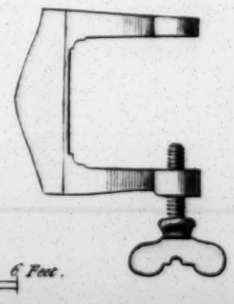
Section of the Pipe & Wheel terminating the Base.



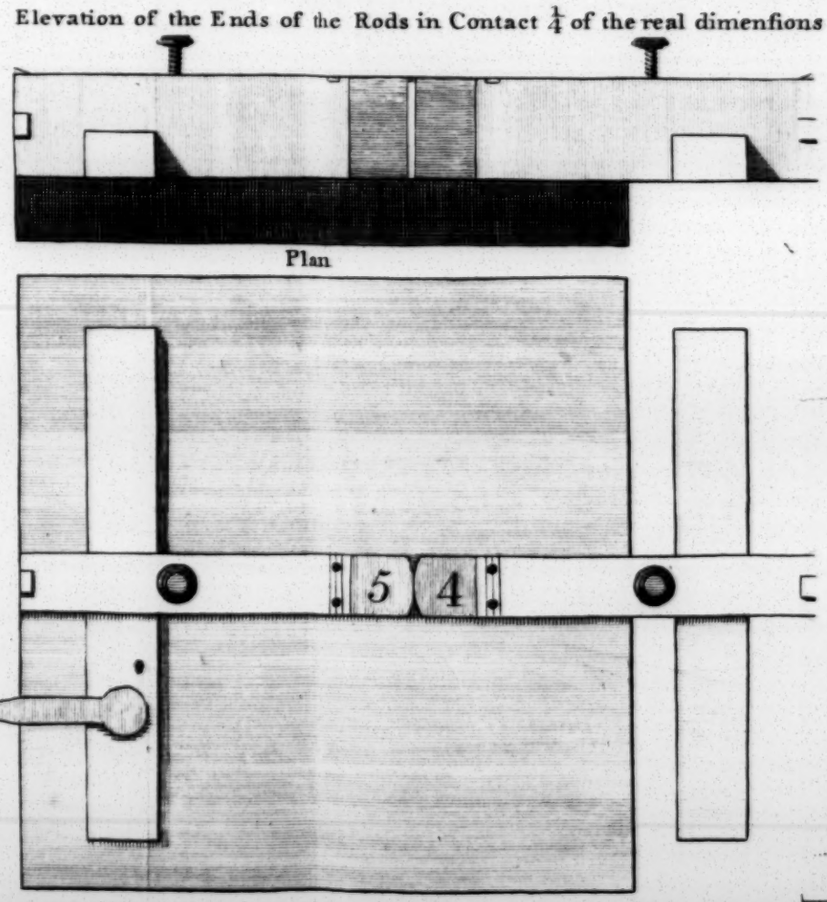
Plan



Vertical Clamp.

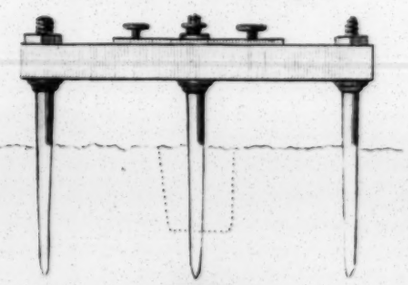


Elevation of the Ends of the Rods in Contact $\frac{1}{4}$ of the real dimensions.

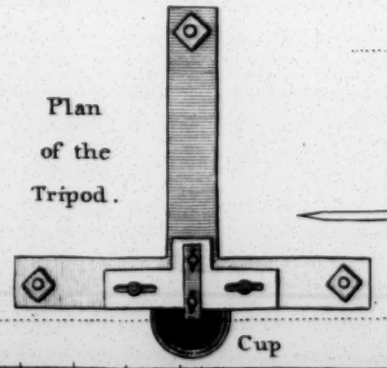


Plan

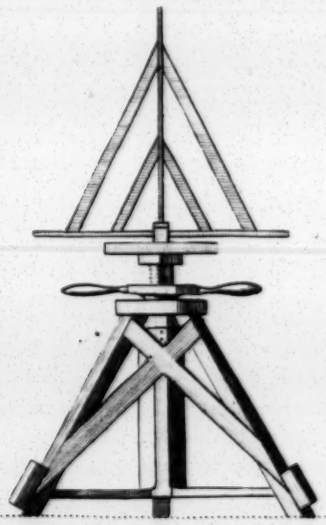
Elevation of the Tripod.



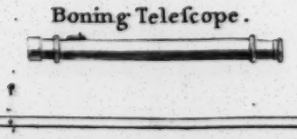
Plan of the Tripod.



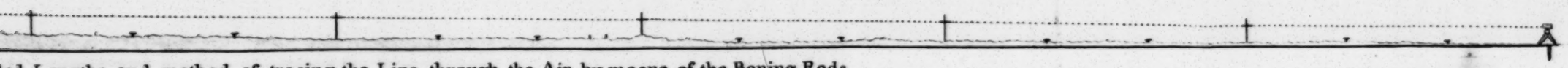
Elevation the Rods seen endw



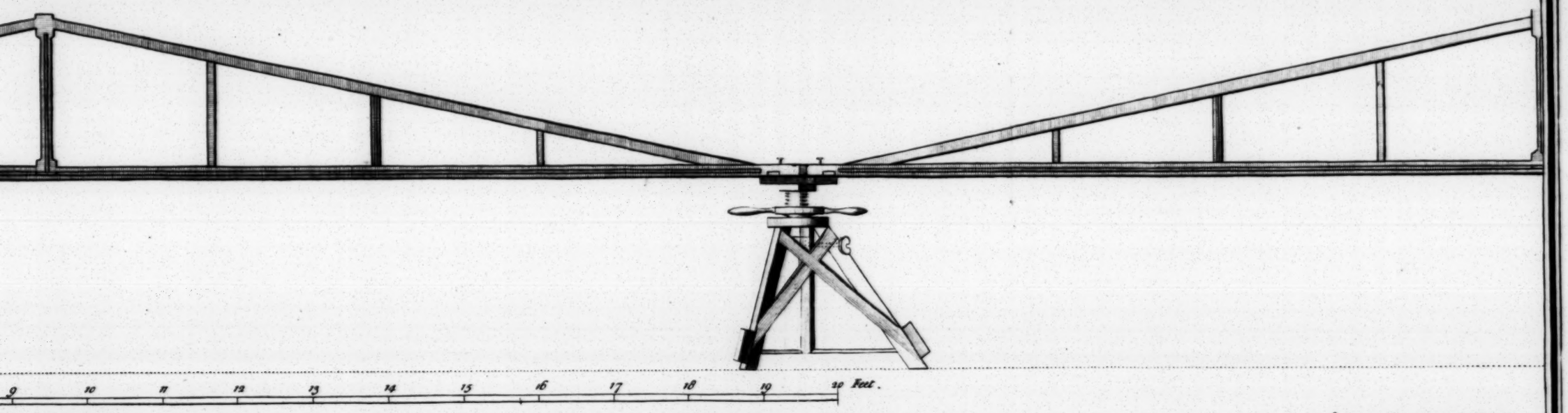
Boning Telescope.



PLICATION of the DEAL RODS &c.

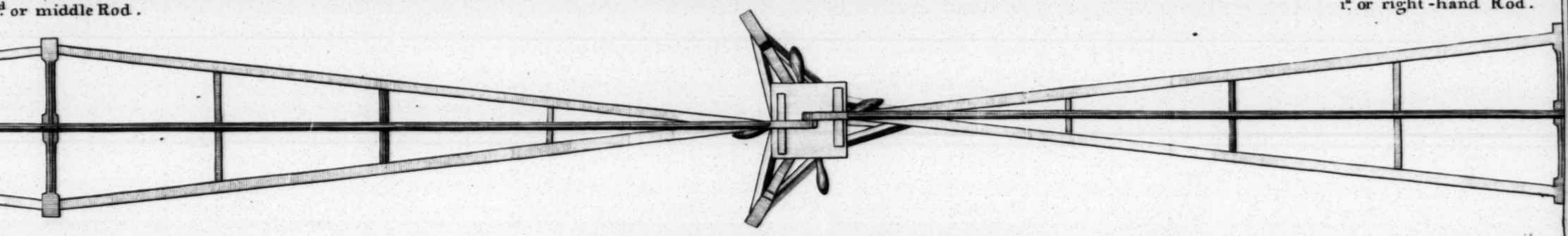


Rod Lengths, and method of tracing the Line through the Air by means of the Boning Rods.



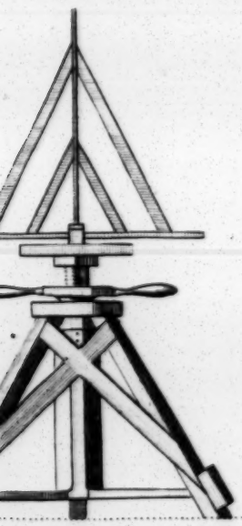
d. or middle Rod.

1st. or right-hand Rod.

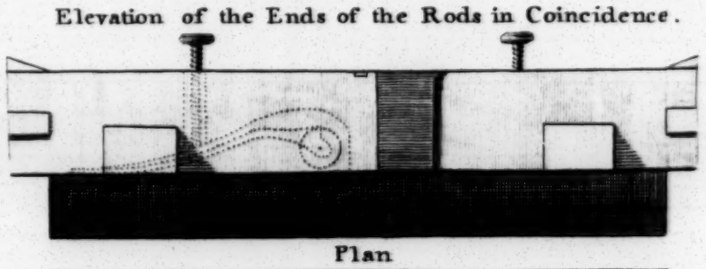


Elevation of the Ends of the Rods in Coincidence.

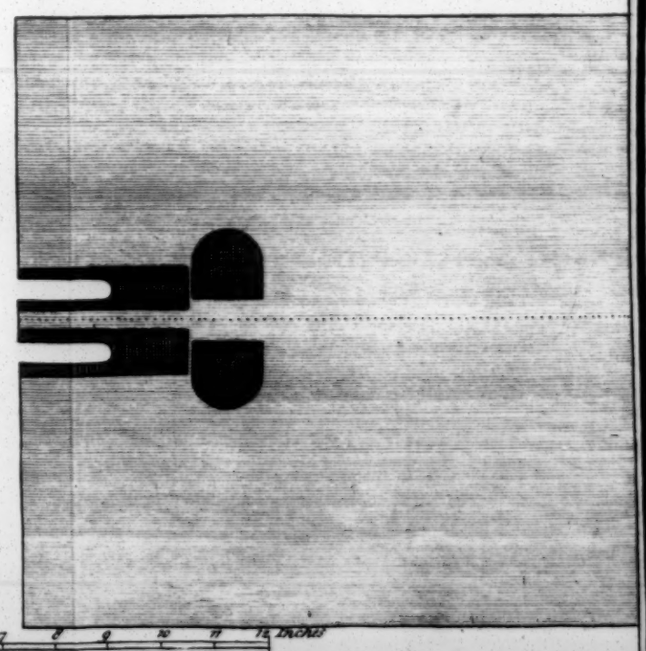
on the Rods seen endwise.



Plan

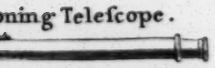


Plan of the Top of the Stand shewing the Grooves for the horizontal Clamp $\frac{1}{4}$ of the real dimensions.

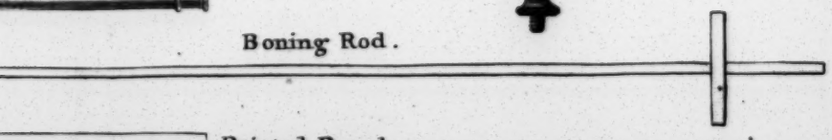


Horizontal Clamp.

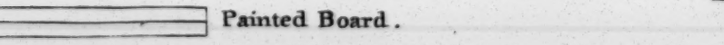
oning Telescope.



Boning Rod.



Painted Board.



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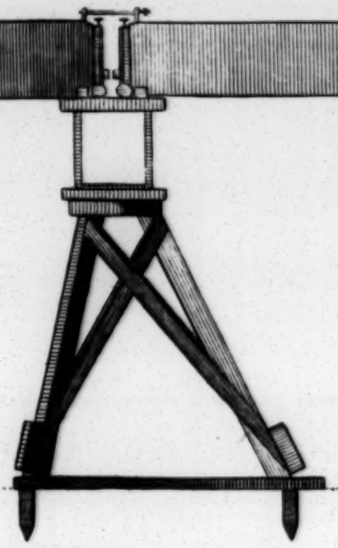
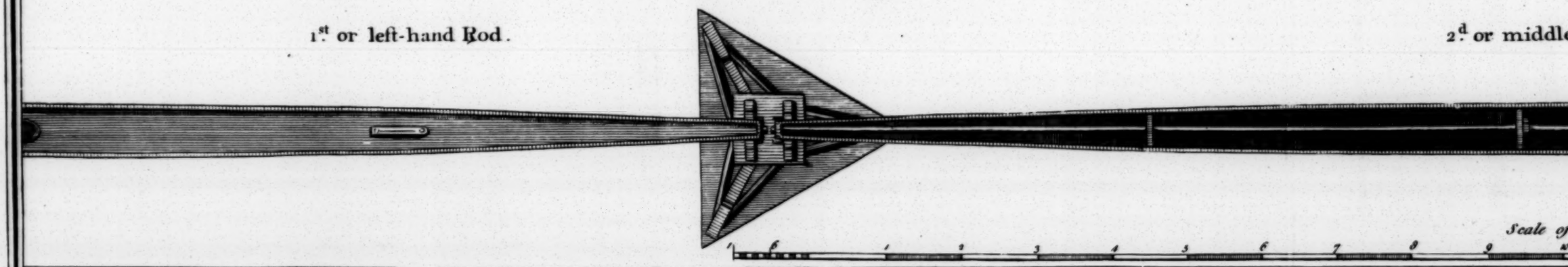
For the DESCRIPTION and APPLIC



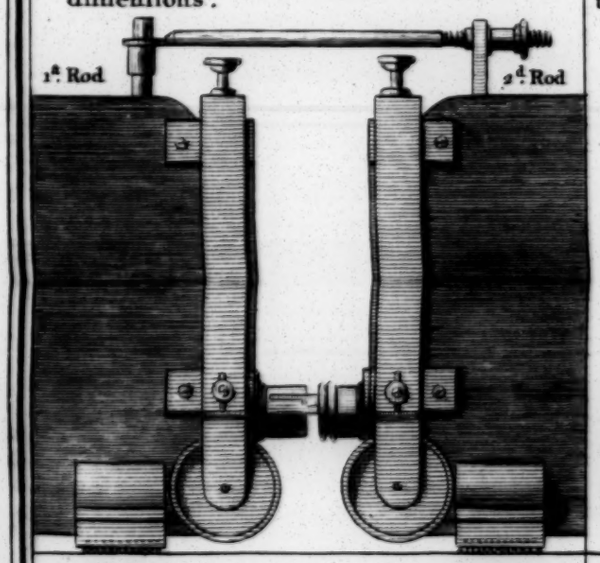
1st or left-hand Rod.



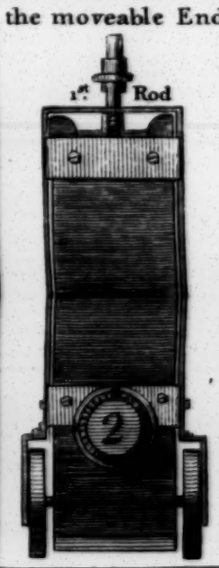
2d or middle

Scale of

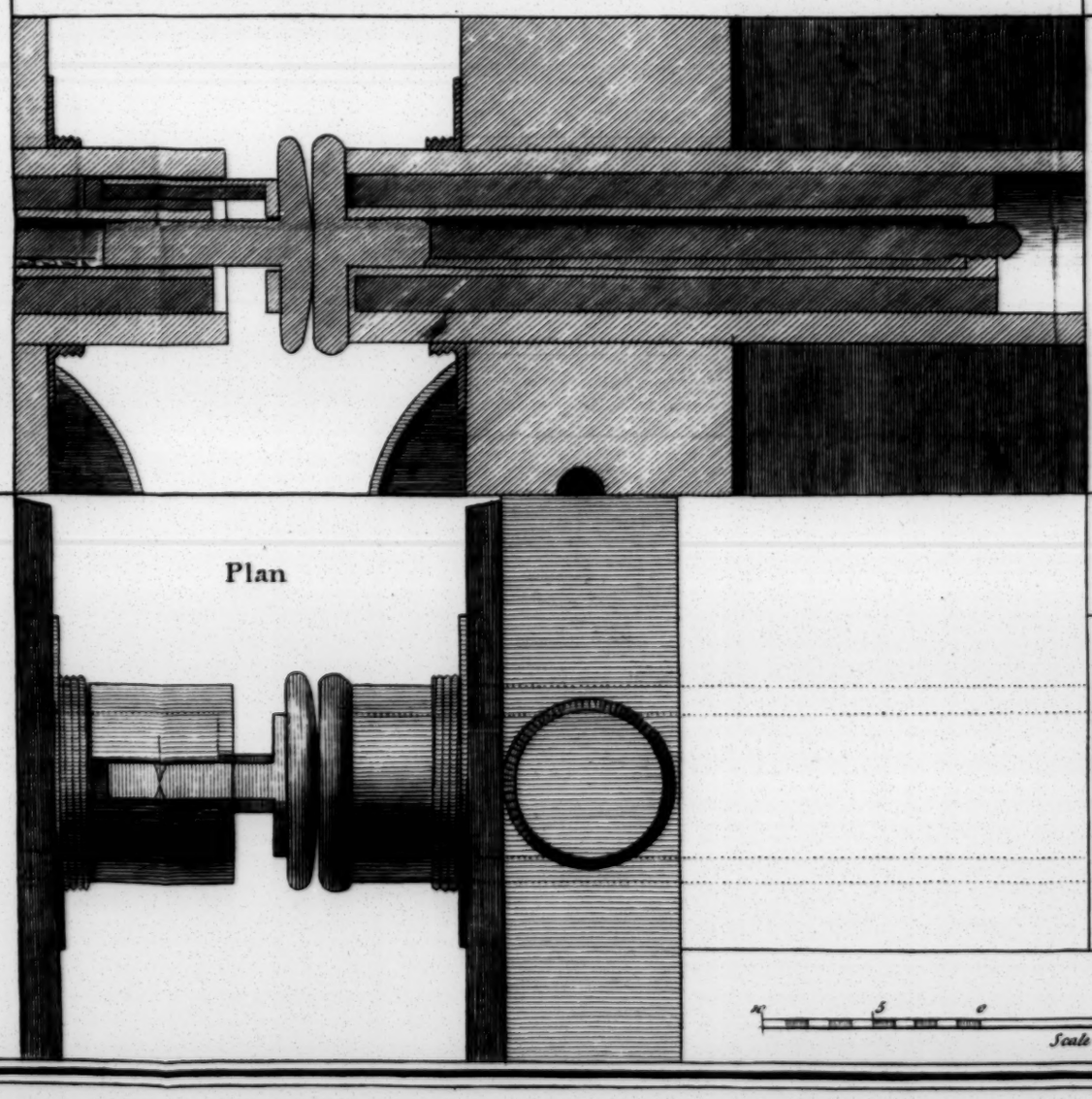
Longitudinal Elevation ¼ of the real dimensions.



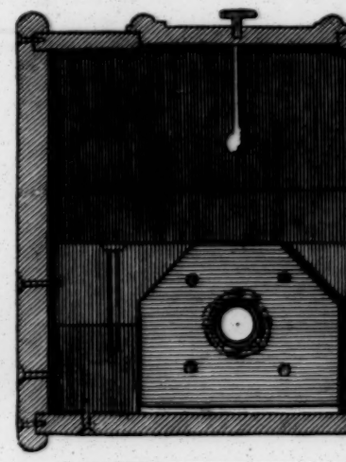
Front Elevation of the moveable End.



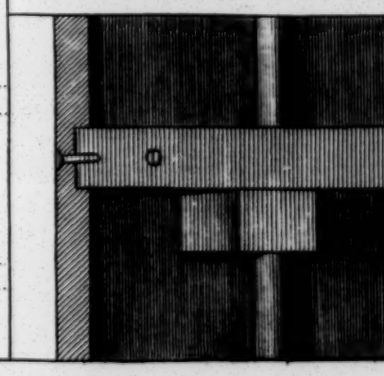
Longitudinal Section of the moveable End of the 1st Rod and fixed End of the 2d Rod in their real dimensions.



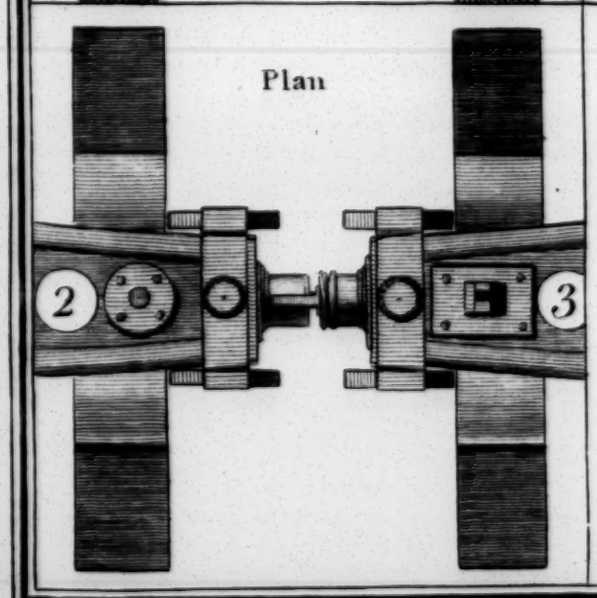
Section across the middle o ¼ of the real dimensions.



Scale of Inches for parts represented in ¼ of th



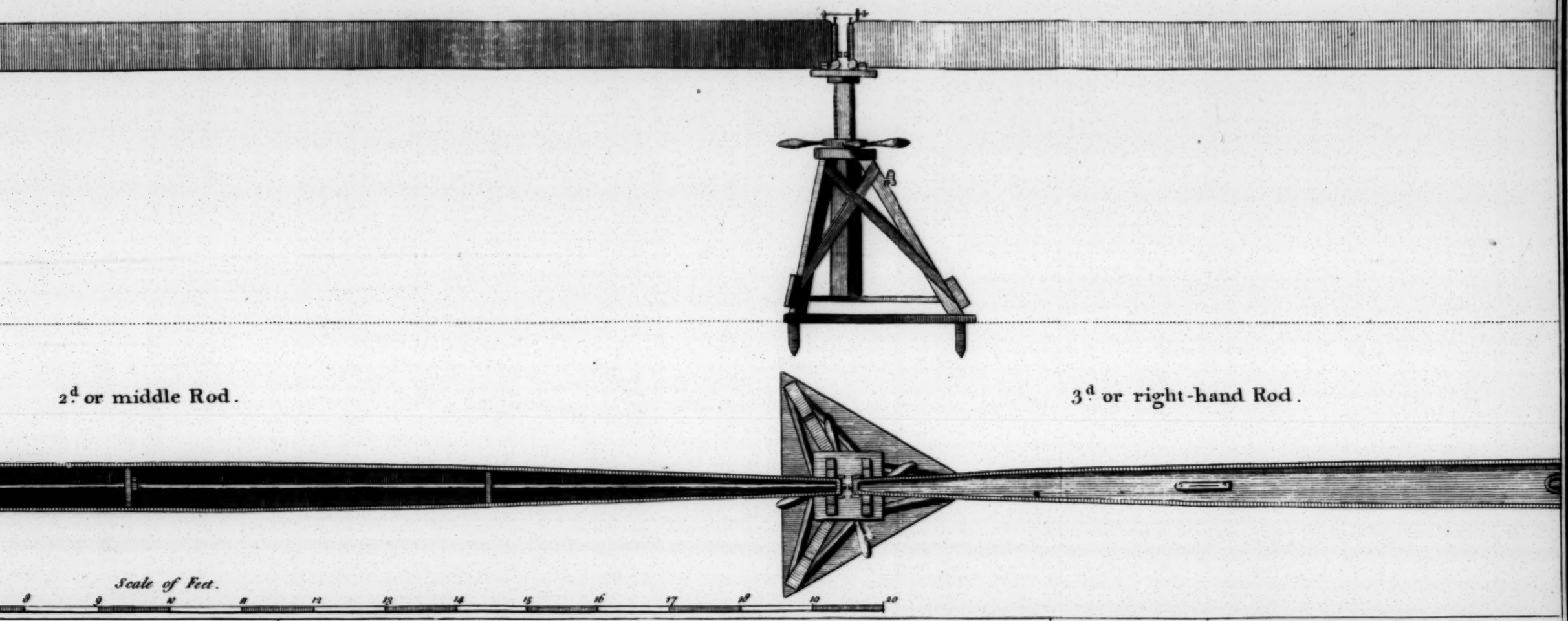
Plan



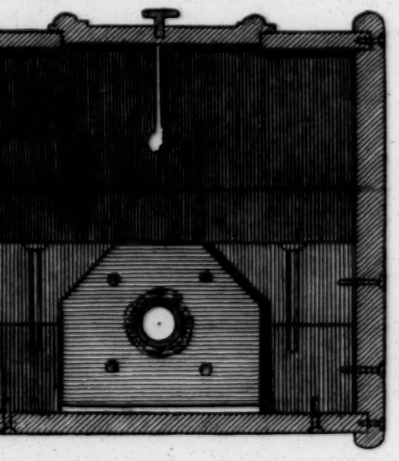
Plan

Scale of Inches for parts represented in their full

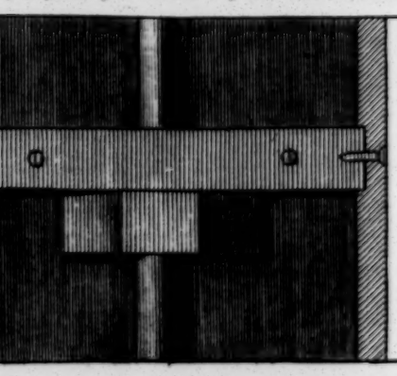
...N and APPLICATION of the GLASS RODS.



... acrofs the middle of the Rod
...e real dimenſions.

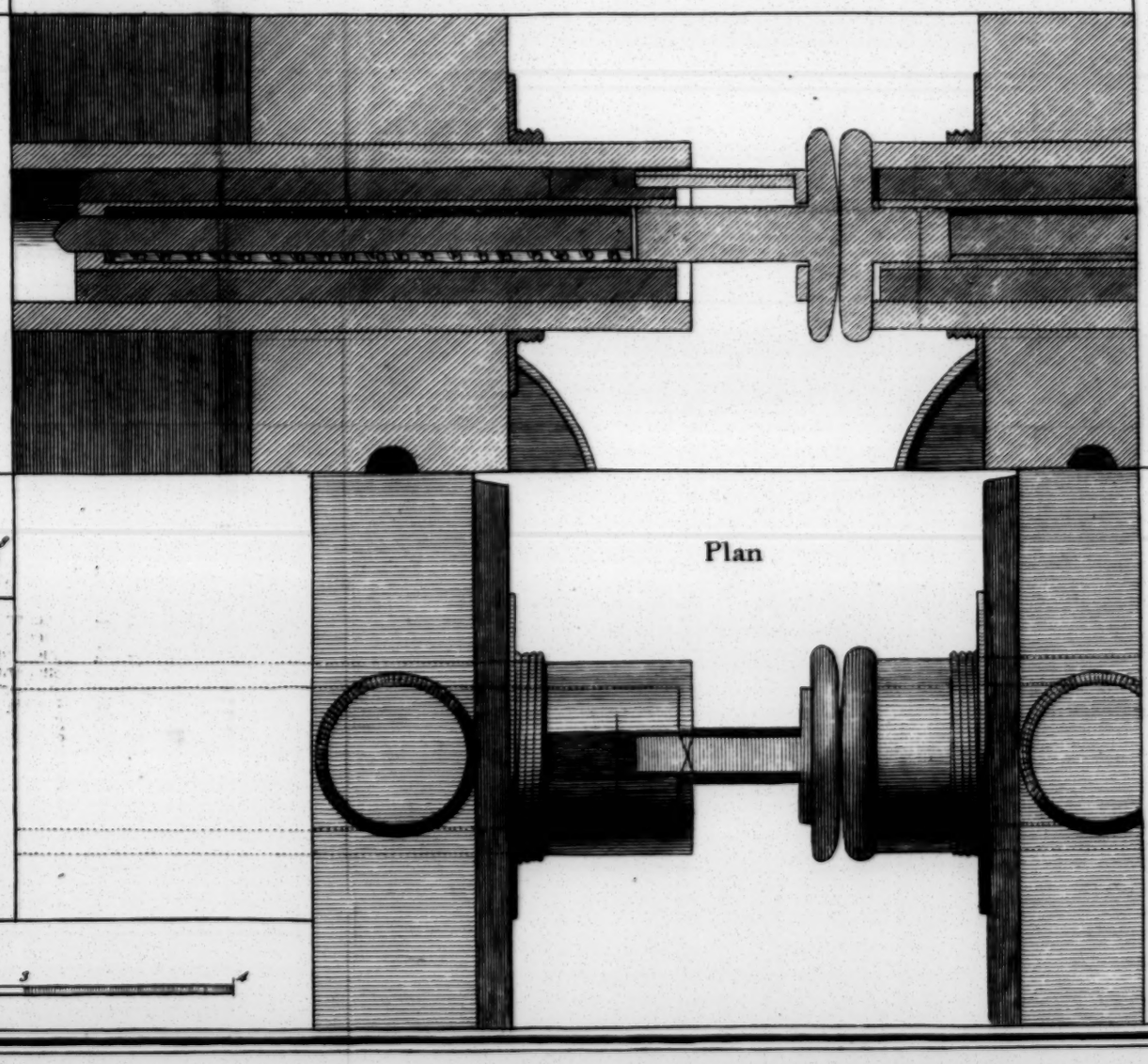


...hes for parts represented in ½ of their real dimensions.

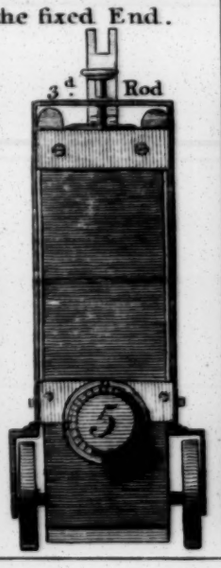


...r parts represented in their full dimensions.

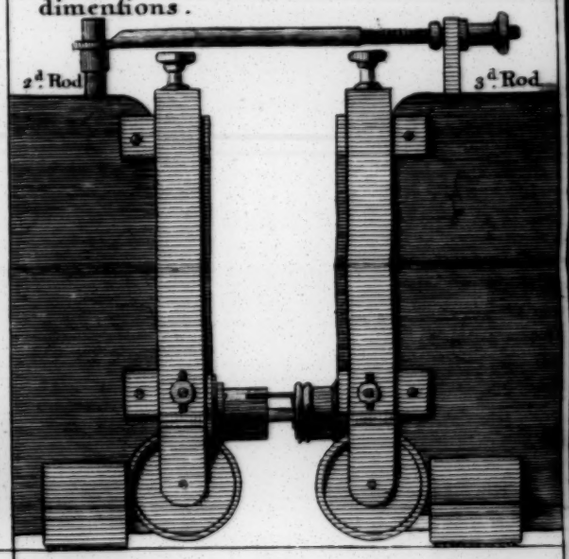
Longitudinal Section of the moveable End of the 2d. Rod and fixed End of the 3d. Rod in their real dimenſions.

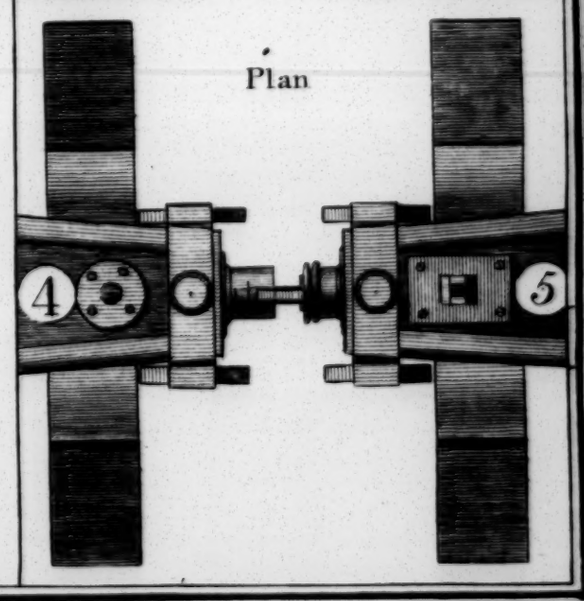


Front Elevation of the fixed End.



Longitudinal Elevation ¼ of the real dimenſions.





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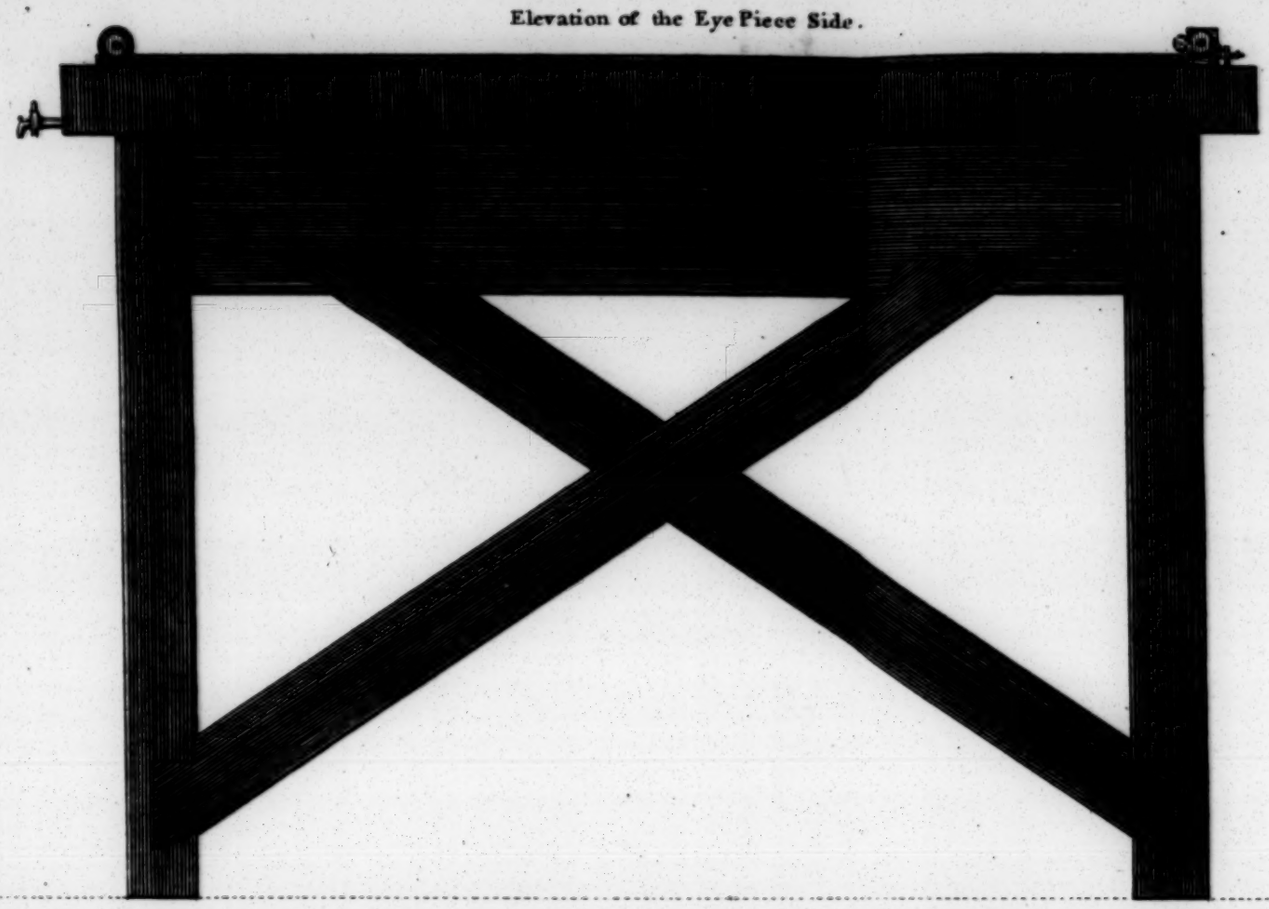
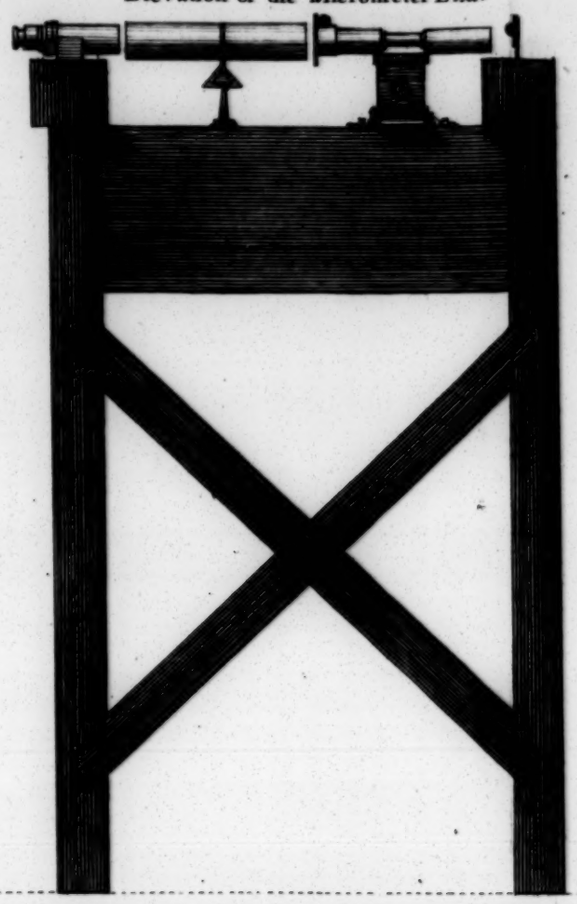
For the DESCRIPTION and USE of the M

Elevation of the Eye Piece Side.

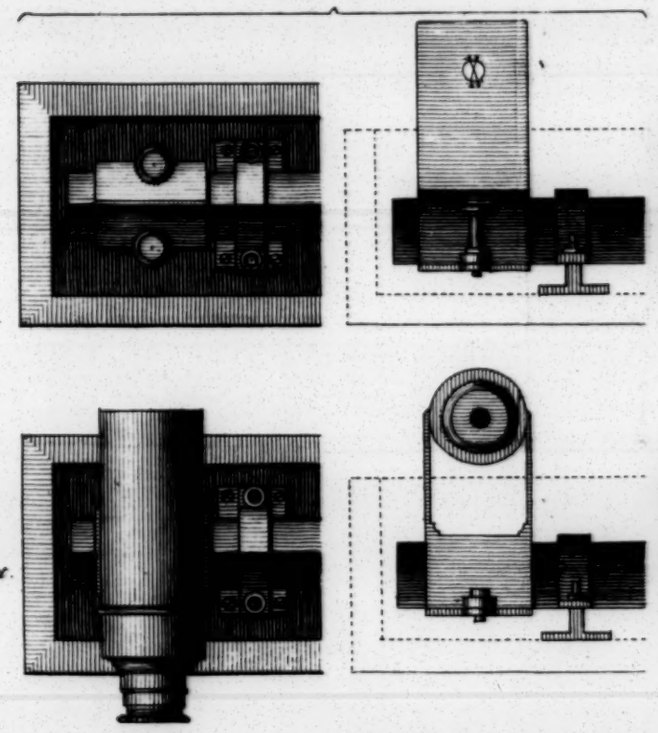


Elevation of the Micrometer End.



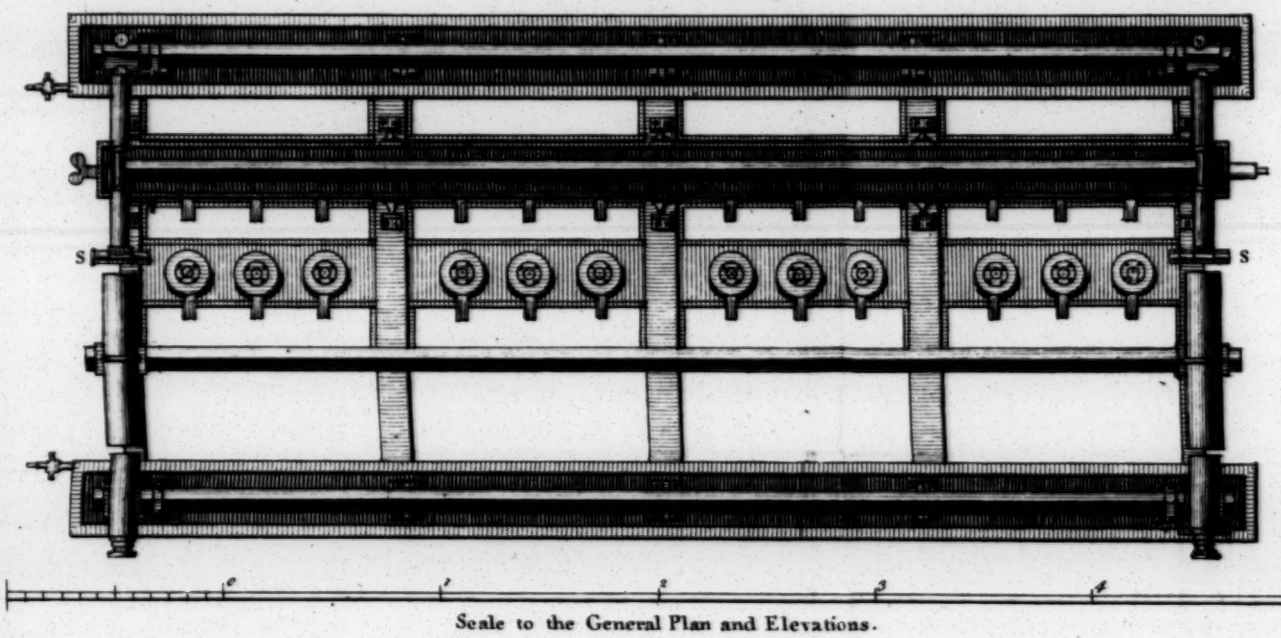
PLANS and ELEVATIONS of the MARKS and EYE-PIECES

Fixed Microscope.



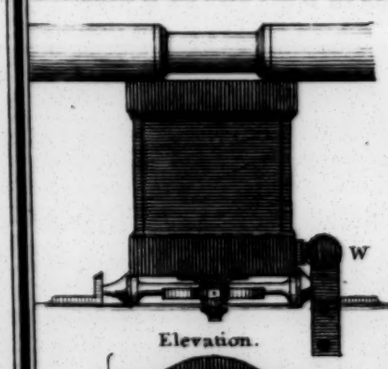
Mi

General Plan of the Top.

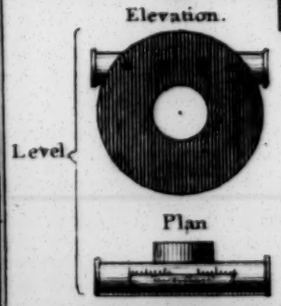


Scale to the General Plan and Elevations.

Elevation of the fixed end of the Boiler.

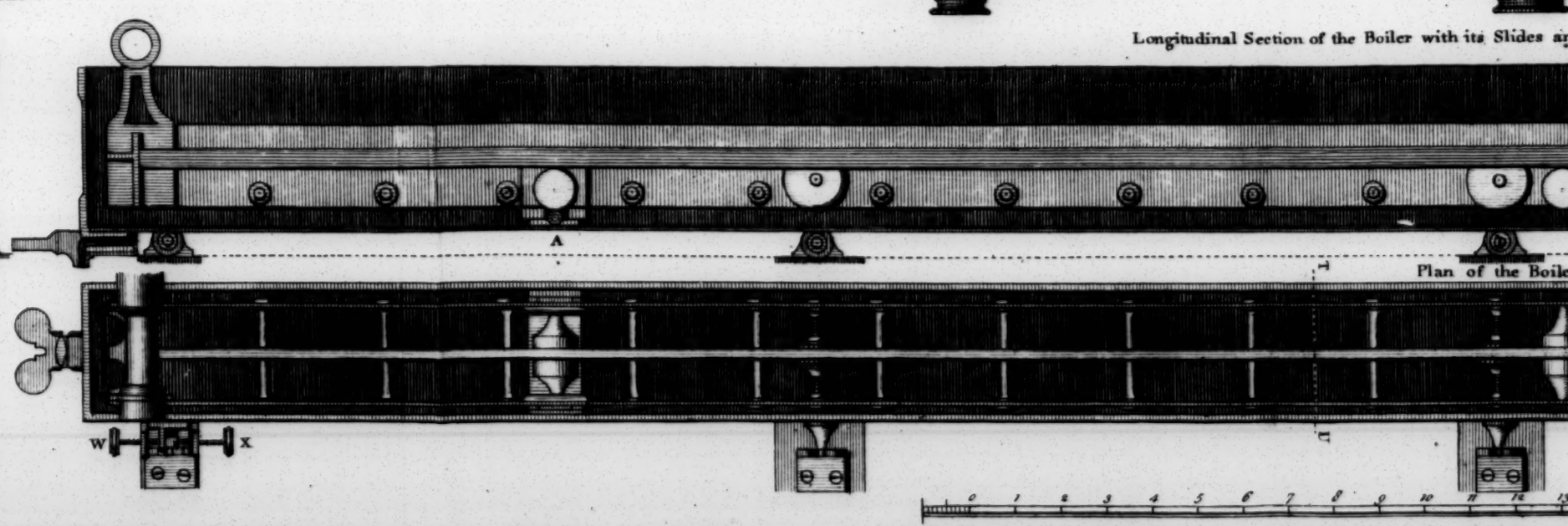


Elevation.

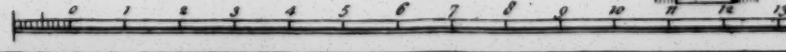


Plan

Longitudinal Section of the Boiler with its Slides a

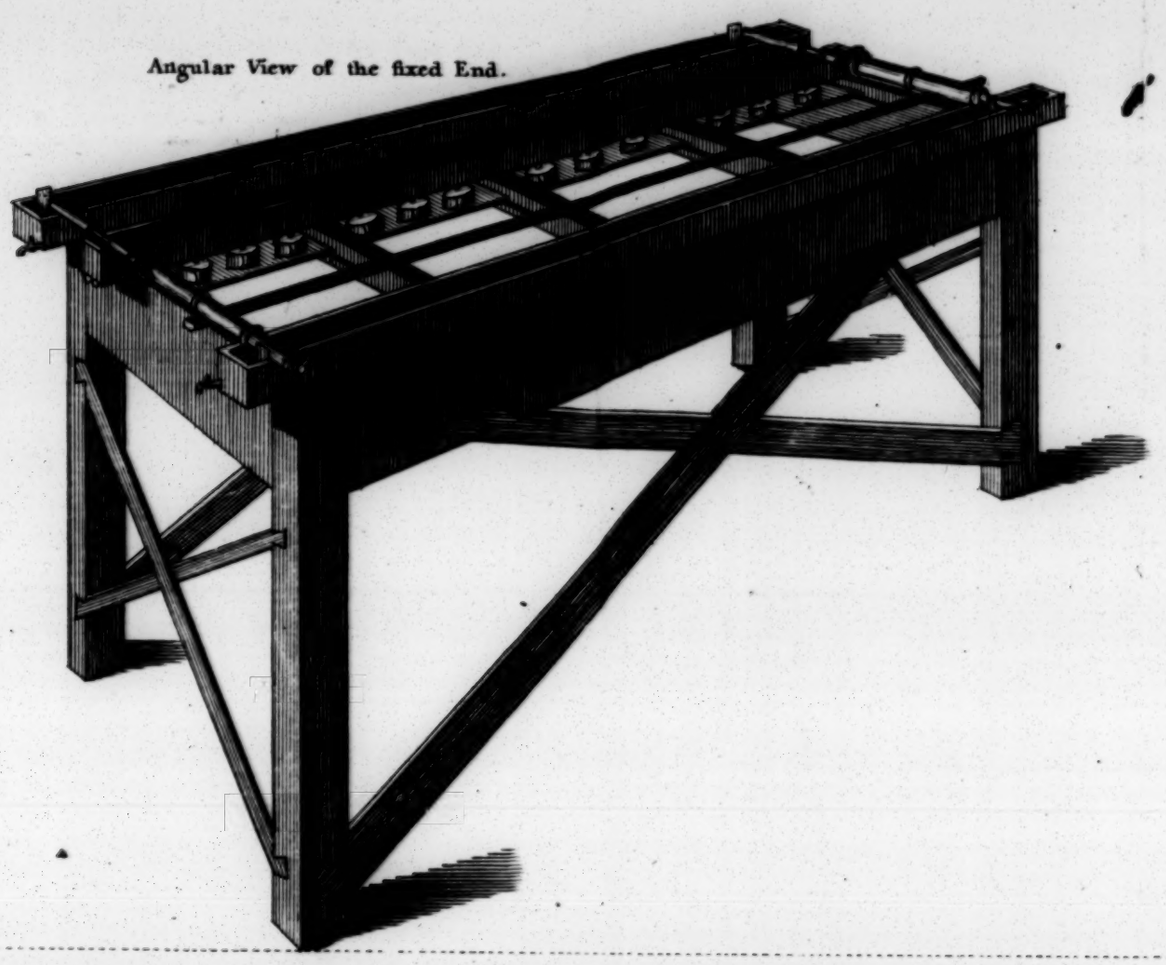


Plan of the Boile

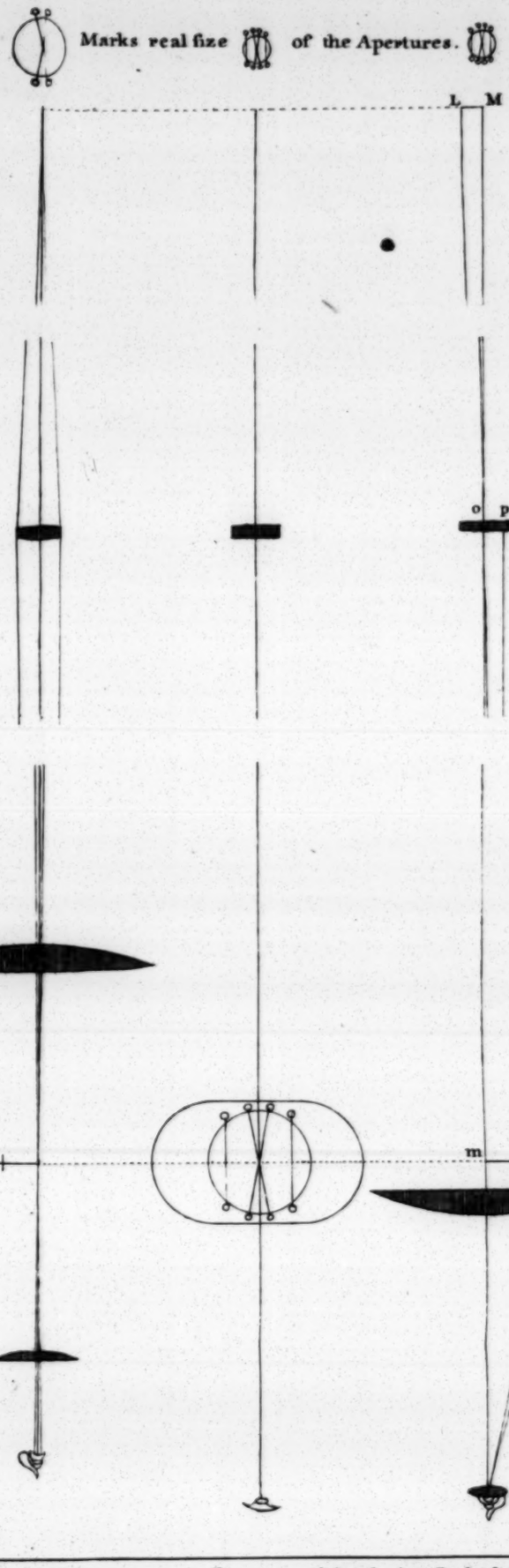


USE of the MICROSCOPIC PYROMETER.

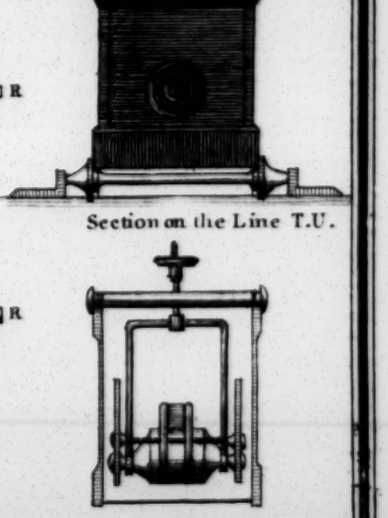
Angular View of the fixed End.



CONSTRUCTION of the MICROSCOPES.



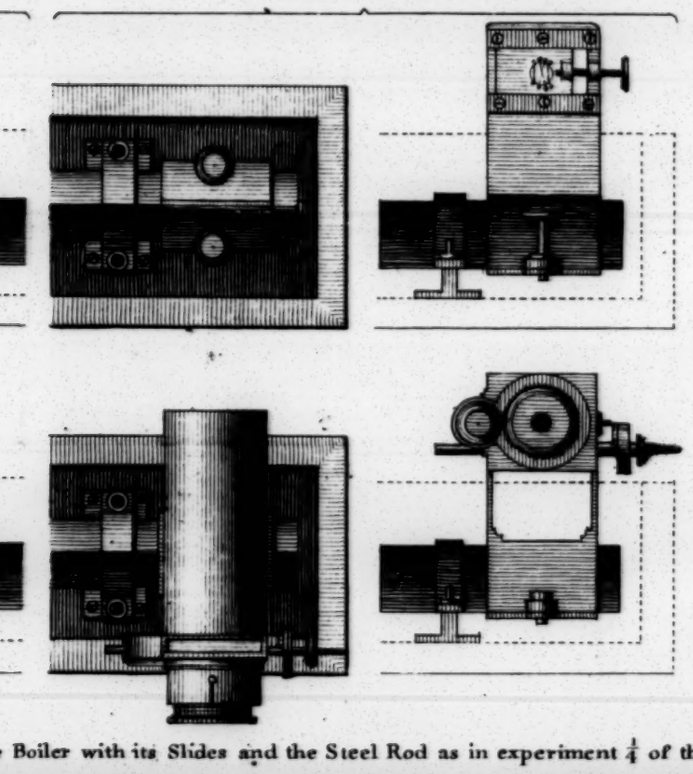
Elevation of the Spring End of the Boiler.



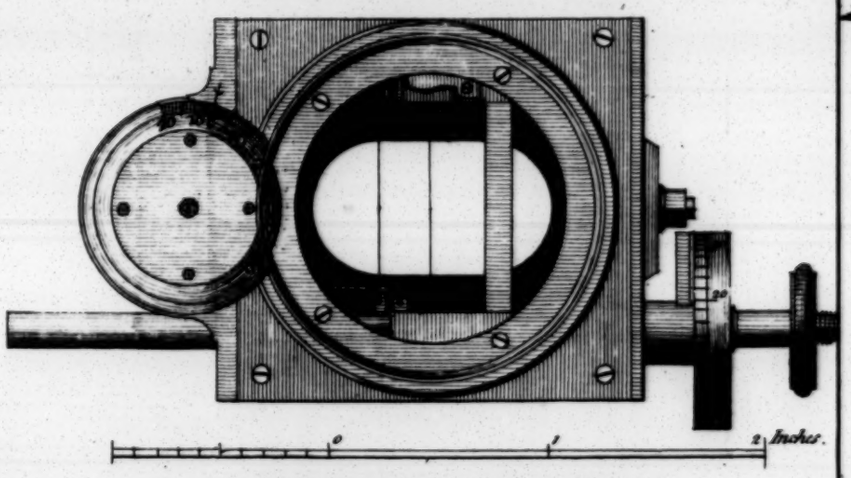
Section on the Line T.U.

ES and EYE-PIECES $\frac{1}{4}$ of the real size.

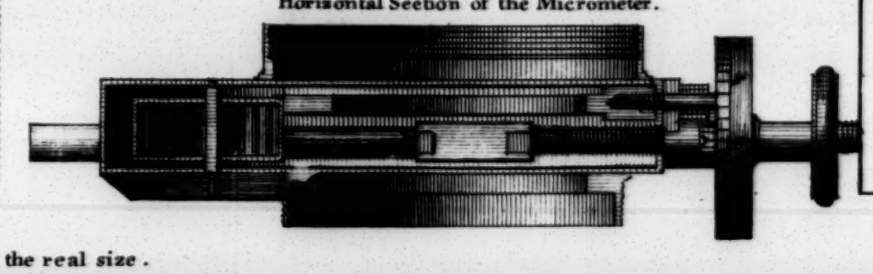
Micrometer Microscope.



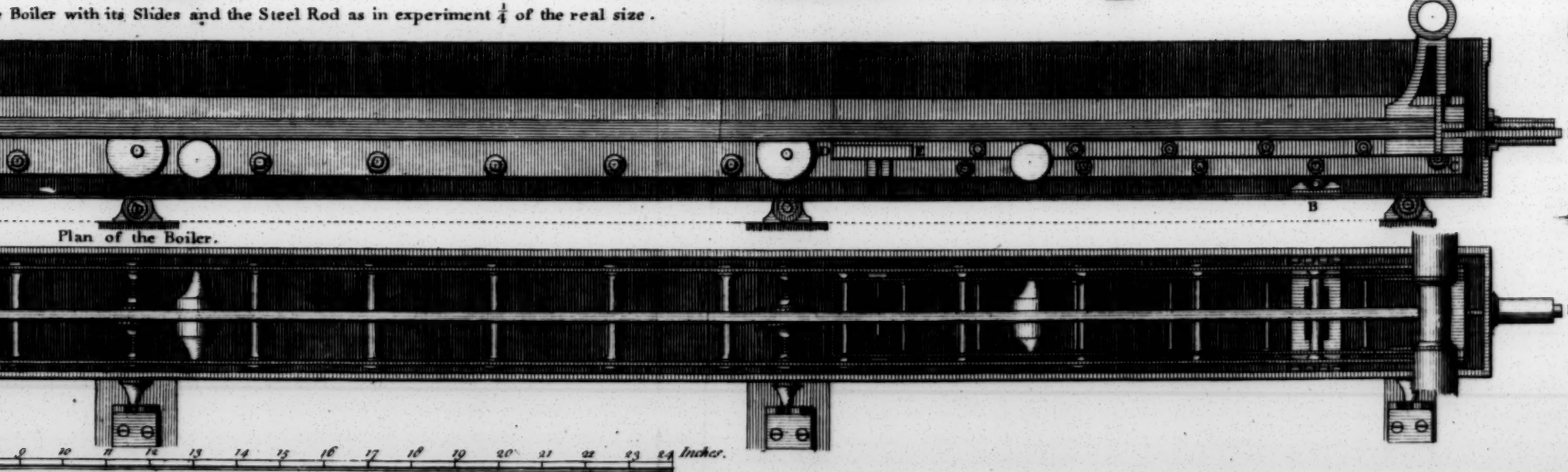
Elevation of the Micrometer full size.



Horizontal Section of the Micrometer.



Boiler with its Slides and the Steel Rod as in experiment $\frac{1}{4}$ of the real size.



Plan of the Boiler.

Basire sc.

Jan.	Mo Afte
Feb.	Mo Afte
Mar.	Mo Afte
Apr.	Mo Afte
May	Mo Afte
June	Mo Afte
July	Mo Afte
Aug.	Mo Afte
Sept.	Mo Afte
Oct.	Mo Afte
Nov.	Mo Afte
Dec.	Mo Afte

XXV. *Abſtract of a Regiſter of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, 1784. *By* Thomas Barker, *Eſq. Alſo of the Rain at* South Lambeth, Surrey; *and at* Selbourn *and* Fyfield, Hampſhire. *Communicated by* Thomas White, *Eſq. F.R.S.*

Read June 16, 1785.

		Barometer.			Thermometer.						Rain.			
					In the Houſe.			Abroad.						
		Higheſt	Loweſt	Mean.	High.	Low.	Mean	High.	Low.	Mean	Lyndon	S. Lambeth, Surrey.	Selbourn, Hants.	Fyfie
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inch.	Inch.	Inch.	Inc
Jan.	Morn.	29,96	28,49	29,34	40½	28	33½	40	15½	27	1,877	2,54	3,18	2,
	Aftern.				42½	29	34	48½	24½	32½				
Feb.	Morn	30,00	28,50	29,23	47	30	36	45	9	29	1,225	1,49	0,77	1,
	Aftern				47½	31	37	52½	23	36				
Mar.	Morn.	29,63	28,59	29,23	47½	36	39½	45	21	32½	1,096	2,63	3,82	2,
	Aftern.				48½	36	40½	52	33½	40½				
Apr.	Morn.	29,74	28,44	29,26	50½	35½	43½	51	29	38½	1,741	2,56	3,92	2,
	Aftern.				53	36	45	60½	35	48				
May	Morn.	29,92	29,17	29,62	66½	47	57½	63	41	52	2,890	1,36	1,52	1,
	Aftern.				69½	48	59½	78	48½	65				
June	Morn.	29,92	28,98	29,43	62	55½	58	61½	48	54	3,810	3,45	3,65	2,
	Aftern.				63½	56½	59½	71	53	62½				
July	Morn.	29,85	28,74	29,48	69	56	61	66	51	56	5,080	2,26	2,40	2,
	Aftern.				72	57	63	79½	57½	67				
Aug.	Morn.	29,92	29,04	29,56	65	54	59	60½	42½	52	2,814	2,84	3,88	2,
	Aftern.				67	55	60	71½	51	63				
Sept.	Morn.	29,90	29,01	29,55	66½	53	61	57	39	52	1,740	1,65	2,51	2
	Aftern.				71½	54	63	73½	51½	64				
Oct.	Morn.	30,00	28,98	29,62	52½	42	48½	45	27½	39½	0,223	0,83	0,39	0
	Aftern.				53½	43	49½	55½	40	49				
Nov.	Morn.	29,85	28,75	29,38	51	39½	45	51½	23½	38	2,376	5,60	4,70	3
	Aftern.				51	41	45½	53	33½	44				
Dec.	Morn.	29,75	28,15	29,26	43½	31½	36½	41	13½	29	2,335		3,06	1
	Aftern.				43	32½	37½	46½	19	32½				
											Inches 27,207	27,21	33,80	2

The froſt, which began at Chriſtmas laſt year, continued, except a few thawing days, till February 21, and was very ſevere, and with frequent ſnow. Some thought it the hardeſt winter ſince 1740; but that may be hard to determine. January 1776 ſeems to have been rather colder than this; but the froſt laſted only a month. In 1780 the froſt was not out of the ground for nine or ten weeks; but it was not ſo ſteady as this. It was certainly one of the mildeſt winters before Chriſtmas, and one of the ſevereſt after it; yet the corn and other plants did not ſuffer ſo much as might be feared. After the froſt there was ſome warm, windy, ſhowery weather; but moſt part of March was froſty mornings, and was often ſo in the ſhade all day; and at the latter end ſtrong cold winds, with ſnow and perfect winter; and it did not much mend till near the middle of April.

While in January and February moſt parts of Europe had ſevere froſt, the ſouthern parts of it ſeem to have had great ſtorms and floods; and, at the breaking of the froſt, the flat countries by the ſides of the great rivers of Europe ſuffered much by floods and ice.

The latter half of April the weather mended, and things came on gradually, yet with frequent froſty mornings till the firſt week in May; then, for three weeks, one of the fineſt and hotteſt Mays ever known; every thing before was exceeding backward, but now came on at a vaſt rate; the graſs and leaves were remarkably green, a great bloſſom year, and plenty of fruit. This hot weather brought up thunder, which turned the weather wet near the end of May, and it was wet or ſhowery and cool all June; this brought on the corn again, which was made rather thin by ſo much heat too early. Near the firſt two thirds of July was again fine and hot, and being

in

in the height of hay time, a good deal of it was well made; but ſome of the firſt cut, and the latter was caught in the wet; for after the 19th it was ſhowery or wet, and the 30th and 31ſt were, all over Leiceſterſhire, Rutland, and part of Lincolnſhire, the greateſt flood ſince July 1736; and it continued wet and cool all Auguſt, ſo that the ſummer was in general cold, wet, and backward, yet with ſome very fine fits in it. The harveſt began but indifferently; but being late this year, and the weather wet, not much was carried before September, when, in about three weeks calm, hot, and dry weather, yet with vaſt dews, moſt of the white corn was well got in this country; but ſome of the peaſe, and, where it was earlier and later, ſome of the white corn was carried damp, for the end of September was again wet.

The autumn was various; a dry and fine October, toward the end of it ſharp froſty mornings; a ſhowery November, with a ſharp froſt in the middle, yet often pleaſant; and after December 5th, a conſiderable ſnow (in ſome countries it was very great) and a ſeverer froſt than is uſual before Chriſtmas laſted till into January.

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